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The Impact of Home Health Prospective Payment on Medicare Service Use and Reimbursement

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EXECUTIVE SUMMARY

As part of its ongoing effort to study methods of providing more cost-effective care, the Health Care Financing Administration (HCFA) implemented the Per-Episode Home Health Prospective Payment Demonstration. Under the demonstration, home health agencies receive a fixed, lump-sum payment for the first 120 days of each episode of care provided to Medicare beneficiaries and a predetermined rate for each visit thereafter. By allowing agencies to retain most of any surplus payments over cost, prospective payment gives agencies a financial incentive to provide home health care in a more cost-efficient manner than under traditional cost-based reimbursement.

DEMONSTRATION OVERVIEW

Ninety-one agencies in five states entered the three-year demonstration at the start of their 1996 fiscal years. Before the start of the demonstration, the participating agencies were randomly assigned to either the treatment or the control group. Agencies assigned to the treatment group were reimbursed under the demonstration's prospective payment method, while those assigned to the control group continued to be reimbursed under cost-based reimbursement (the payment method Medicare used for all home health agencies when the demonstration began).

For each patient admitted to home health care, treatment group agencies received a lump-sum payment for the first 120 days of home health care, regardless of the number of visits provided or their cost. (Durable medical equipment, nonroutine medical supplies, and Part B ambulatory services continued to be reimbursed at cost.) The amount of the per-episode payment was based on each agency's own costs in the fiscal year immediately preceding its entry into the demonstration, adjusted for changes in its case mix and for inflation. An agency could receive a new per-episode payment for a given Medicare beneficiary only after the 120-day "at-risk" period had ended and a 45-day gap in services had taken place. For each visit after the at-risk period that did not begin a new episode, treatment agencies received a fixed payment that varied by type of visit and was again based on the agency's own predemonstration costs.

HYPOTHESES AND METHODS

In this report, we examine data from the first two years of the demonstration to test hypotheses about the possible effects of prospective payment on the use of Medicare-covered services by agency patients and on reimbursement for those services. We tested hypotheses concerning the impacts of the demonstration on the use of and reimbursement for Medicare services by type. These types of services were emergency room, inpatient hospital, skilled nursing facility, hospice, nondemonstration home health, outpatient hospital, physician and other practitioner, durable medical equipment, and other Part B services. Our basic hypothesis was that reductions in home health visits (or in the cost of providing visits) could have adverse patient outcomes that would be reflected in increases in the use of other Medicare services. (The impacts of the demonstration on the use of home health

services delivered by demonstration agencies and on the cost of delivering those services are the subjects of other evaluation reports.)

Our main analysis centers on patients admitted to an agency during its first year in the demonstration, because we were able to observe their subsequent use of Medicare services over an extended period of at least 12 months. In supplementary analysis (based on shorter periods of service use), we then compared impacts for key outcomes for the first and second demonstration years. The sample for the main analysis included approximately 57,000 home health patients admitted to one of 87 demonstration agencies. (Three of the 91 original agencies left the demonstration near its start and thus were excluded from our analysis. A fourth agency was excluded because it had too few admissions to provide reliable estimates of its patients' service use in a typical year.)

Data for our analysis come from several sources. Medicare claims files provided data for outcome variables--patients' use of and reimbursement for services during the year following home health admission--as well as for measures of service use during the six months before admission. Data collected at admission for demonstration case-mix adjustment provided measures of preadmission characteristics of patients. Data from the demonstration's quality assurance contractor provided nurses' assessments of patients at home health admission (but for only a subset of patients in our analysis). Data on agency characteristics were obtained from agency cost reports and the demonstration's implementation contractor, while data on area characteristics were obtained from the Area Resource File.

Ordinary least squares models and logit models were used to estimate program effects, controlling for preexisting differences between treatment and control agencies in patient, agency, and area characteristics. This approach proved crucial to obtaining valid impact estimates because, despite the random assignment of participating agencies, there were several significant differences between treatment and control agencies aside from the method of payment. Observations were weighted so that each agency was represented equally in the analysis. Standard errors of impact estimates were calculated using special software designed to account for the effects of sample clustering and weighting, to avoid overstating the precision of the estimates. Analyses of the robustness of our regression estimates showed that very few of these estimates were sensitive to the weighting scheme or statistical methods used.

FINDINGS

Prospective payment led to a 25 percent reduction in home health visits and episode length over the year following admission to a demonstration home health agency. However, these reductions did not appear to lead to an overall increase in the use of other Medicare services during that year. We did observe a moderate increase (three percentage points) in the likelihood of treatment agency patients using nondemonstration home health services. Given the absence of increases in other indicators of adverse patient outcomes, however, we concluded that the higher rate of nondemonstration home health use did *not* suggest that earlier discharges from demonstration agencies had been inappropriate. Total Medicare expenditures during the year following admission (other than for demonstration home health services) were similar for patients of treatment and

control agencies. This absence of an increase in the use of other Medicare-covered services suggests that a reduction in home health use at the level observed under the demonstration does not adversely affect care quality or shift costs to services in other settings. The following chart summarizes the effects of prospective payment on key Medicare service use and reimbursement outcomes during the year after a demonstration home health admission for admissions occurring during the first demonstration year. (The effects were strikingly similar for admissions in the second demonstration year.)

Outcome per Patient, During the Year Following Home Health Admission, for Patients Admitted During Demonstration Year 1	Control Group Mean	Treatment-Control Difference	*p*-Value
Number of emergency room encounters	1.27	−0.09***	0.01
Number of inpatient hospital admissions	1.09	−0.04	0.21
Any skilled nursing facility admission (percent)	20.0	−0.1	0.88
Any nondemonstration home health admission (percent)	16.9	2.8**	0.02
Medicare Part A reimbursement, exclusive of demonstration home health (dollars)	11,292	−168	0.62
Medicare Part B reimbursement (dollars)	4,864	−37	0.79

**Means statistically different from zero at the .05 level, two-tailed test.

***Means statistically different from zero at the .01 level, two-tailed test.

Emergency Room Use Appeared to Decline Slightly

A key concern about the implementation of per-episode payment was that the quality of home health care might suffer as a result of financial incentives to reduce the number of visits provided or the cost of providing visits. The most serious adverse effects on quality would be reflected in increases in emergency room and hospital services. During the year following home health admission, the use of these services was high relative to the general Medicare population: 55 percent of control agency patients had at least one emergency room encounter, and 52 percent were admitted to the hospital.

Instead of seeing an increase in use, however, we observed small, but statistically significant, reductions in emergency room encounters for treatment agency patients during the year following home health admission. (The chart above shows a seven percent reduction in the total number of emergency room encounters.) Site visit interviews suggested that this reduction may have resulted from treatment agency nurses' efforts to improve patient self-care in order to facilitate earlier discharge from home health. Reductions in emergency room encounters included reductions in encounters that led to inpatient admissions. This, in turn, appeared to lead to a pattern of small, negative treatment-control differences in inpatient services during the year following home health admission.

Use of Skilled Nursing Facility and Hospice Services Was Not Affected

Even if per-episode payment did not adversely affect care quality, it might have caused care to be shifted from home health to some other setting (such as a nursing home or hospice), particularly if a home health agency viewed a patient as substantially more costly to serve. However, we found no evidence that care in skilled nursing facilities or hospices was substituting for the observed reduction in home health visits. During the year following home health admission, about 20 percent of patients from treatment and control group agencies were admitted to a nursing facility and 6 percent were admitted to a hospice.

Use of Nondemonstration Home Health Services Increased Somewhat but Did Not Reflect Adverse Patient Outcomes

A number of events could lead a patient of a treatment home health agency to receive services from another home health agency during the year following admission. Some of these events are not related to the demonstration and thus should be equally likely to occur to patients of control agencies. For example, hospital-based home health agencies sometimes provide services to patients who require home health care following hospitalization, even if the patient had been in the care of another agency before the hospitalization. On the other hand, the earlier discharge of patients by treatment agencies, a clear incentive under per-episode payment, is an event that may lead treatment agency patients to use more nondemonstration services than control agency patients.

Treatment agency patients were more likely than control agency patients to have a nondemonstration home health admission during the year following a demonstration admission: 20 percent were admitted to a nondemonstration agency, compared with 17 percent of control agency patients. If treatment group agencies' earlier discharges had been inappropriate, we would have expected to see increases in emergency room, inpatient, or skilled nursing facility service use. We might also have expected to see nondemonstration agencies providing more visits to treatment agency patients (relative to control agency patients) to compensate for these discharges. We saw no such increases, however.

The higher rate of nondemonstration home health admission among treatment agency patients seems to have been related to two factors. First, because treatment agency patients were discharged earlier than control patients from demonstration agencies, they had a greater window of opportunity to have nondemonstration agency admissions in the follow-up year. That is, control agency patients still receiving demonstration home health services could not have been admitted to other home health agencies (except in rare instances of patient sharing), whereas patients discharged earlier by treatment agencies were at risk of such admissions. Second, a few treatment group agencies whose patients had unusually high nondemonstration admission rates (but *not* unusually short demonstration episodes or low numbers of visits) seemed to have driven the treatment-control difference.

Use of Part B Services and Overall Reimbursement Were Not Affected

Per-episode payment had no effect on the use of Part B services. Nearly 80 percent of patients from treatment and control agencies had an outpatient encounter during the year following home health admission, often for hospital-based lab tests or X rays. Nearly all (96 percent) saw their physicians or other practitioners; more than half (56 percent) purchased durable medical equipment; and most (94 percent) used other Part B services, frequently for lab tests and X rays from freestanding providers.

Because per-episode prospective payment had modest, but offsetting, effects on the use of Medicare services (small reductions in emergency room and inpatient use but a modest increase in nondemonstration home health use), it had no effect on overall Medicare spending. On average, patients of treatment and control agencies had Part A reimbursement (exclusive of payment to demonstration home health agencies) of about $11,200 and Part B reimbursement of about $4,800 during the year following home health admission, for a total of about $16,000 (or about $1,300 per month).

Estimates of the effect of per-episode payment on demonstration home health reimbursement--the cost of care to the Medicare program--are not informative, because the payment mechanisms for patients of treatment and control agencies were not comparable, especially during the at-risk period. We note, however, that reimbursement for home health services provided by demonstration agencies during the year following admission averaged $5,050 for patients of control group agencies, compared with $4,439 for patients of treatment group agencies. (The treatment group mean is a composite of a raw mean for the initial four-month, at-risk period and a regression-adjusted mean for the subsequent eight months.) While there was almost no difference in demonstration home health reimbursement during the at-risk period, the regression-adjusted difference for the rest of the year was large and statistically significant. The difference during months 5 through 12 is a result of the fact that a much smaller number of treatment agency patients were still receiving visits from demonstration agencies after the at-risk period.

We noted earlier that a somewhat higher proportion of treatment agency patients used nondemonstration home health services during the year following demonstration agency admission. However, the cost of these nondemonstration home health services for treatment agency patients was not high enough to offset the higher cost of demonstration home health care for control agency patients. Total demonstration and nondemonstration home health reimbursement was just under $6,100 for patients of control agencies and just under $5,700 for their treatment group counterparts. Thus, total Medicare spending for treatment agency patients (including spending for demonstration home health) was somewhat lower than spending for control agency patients.

CONCLUSION

Per-episode payment led to a substantial reduction in the number of home health visits provided by treatment group agencies. However, we found no evidence that these reductions adversely affected patients or led to the substitution of care in other settings, as reflected in the use of and reimbursement for other Medicare-covered services. These conclusions were consistent under

alternative weighting schemes and statistical methods for estimating program impacts, underscoring their robustness. Moreover, they are consistent with those of the evaluation's analysis of demonstration impacts on care quality, an analysis that examined data from the demonstration's quality assurance contractor and data describing the use of Medicare services for specific home health admitting diagnoses (Chen 1999).

Our study has some limitations, despite the robustness of its conclusion that the reduction in home health visits observed under per-episode prospective payment did not lead to adverse patient outcomes or cost shifting as reflected in the use of other Medicare services. Perhaps the most important potential limitation is the extent to which our findings may be generalized to home health agencies nationwide. As in any voluntary demonstration, the agencies that chose to participate may reflect a group better able to reduce visits while maintaining care quality. If they did, our demonstration impacts may not accurately reflect changes in Medicare service use that would take place nationally under a similar payment system.

However, we believe that the results of this analysis can be generalized because overall findings and findings for different subgroups of patients were largely consistent. Agencies of differing size, auspice, or practice pattern might be expected to differ in their ability to cut visits while maintaining care quality. However, even patients of agencies that made the deepest cuts to visits did not appear to suffer adverse outcomes, as reflected in their use of other Medicare services. Thus, even if the mix of demonstration agencies differed from agencies nationwide, we might expect a nationwide program to be able to substantially reduce home health visits while not adversely affecting patients so that they increased their use of other Medicare services. However, if home health agencies nationwide made deeper cuts to visits than we observed under the demonstration, it is unclear at what point such cuts would lead to adverse patient outcomes.

A second limitation, related to the issue of generalizability, is that the national program of prospective payment differs from the one implemented for the demonstration. For example, agencies are not protected from incurring financial losses. This could lead some agencies to respond more aggressively to the program incentive to reduce visits or costs, to be more certain of avoiding losses. If agencies make larger reductions in visits than those observed under this demonstration, at some point these reductions are likely to have an adverse effect on patients. However, no research has been done to determine the critical threshold for such reductions.

A final limitation is that this report does not provide information on all the consequences of reducing services, only on those serious enough to be reflected in the use of other Medicare services. Other project reports examine the effects of the demonstration on other aspects of care quality, patient selection and retention, and the use of informal care and services paid by funders other than Medicare. In addition, the evaluation will examine the impacts of the demonstration on agency cost per visit and cost per episode, as well as on profit and loss. A final report will combine the findings across outcome measures.

The evidence this analysis provides clearly shows that the reductions in home health visits observed under the per-episode prospective payment demonstration did not adversely affect patients in ways that were reflected in the increased use of other Medicare services or cause patients to shift care to other settings. However, the analysis also underscores the vulnerability of Medicare home

health patients. More than half the patients in our analysis were admitted to a hospital during the year following their demonstration home health admission, compared with just under 20 percent of all Medicare enrollees in 1996 (U.S. Social Security Administration 1998). Total Medicare expenditures for patients of demonstration agencies were over $20,000 per person during the year, more than quadruple the spending for all enrollees during 1996 (Health Care Financing Administration 1998). In addition, a recent AARP study found that 43 percent of Medicare beneficiaries who had used health home services in 1997 had at least one limitation in daily living activities and 33 percent lived alone, compared with 10 and 25 percent, respectively, of all Medicare enrollees. Their out-of-pocket health care spending was 27 percent of their income, compared with just 18 percent for all Medicare enrollees (Foley et al. 1998).

These statistics paint a picture of the Medicare home health patient population as much more medically fragile, functionally limited, and socially isolated than the typical Medicare enrollee, as well as having higher out-of-pocket health care costs. Therefore, while it seems clear that the reduction in home health visits observed under the demonstration did not increase patients' use of other Medicare services, it is unclear how much larger the reduction could have been without doing harm. Because Medicare home health patients are so vulnerable, caution should accompany the setting of policy parameters that could induce agencies to cut home health visits even more deeply. Moreover, now that prospective payment has become law, as agencies develop more experience with it, HCFA will need to monitor both agencies' responses to the incentive to cut visits and the effects such cuts have on beneficiaries and their use of other Medicare services.

I. THE PER-EPISODE HOME HEALTH DEMONSTRATION AND EVALUATION

The Health Care Financing Administration's (HCFA's) Per-Episode Home Health Prospective Payment Demonstration tested the extent to which prospective payment for Medicare home health services increases the efficiency of service provision. This efficiency is meant to reduce public expenditures while maintaining access to and quality of care. Per-episode payment encourages efficiency by giving agencies the financial incentive to reduce their costs. Specifically, under the demonstration payment system, any savings that produce lower costs per episode of patient care may generate profit for an agency.[1] These incentives differ greatly from those under cost-based reimbursement (the payment method Medicare used for home health when the demonstration began), which provided no reward for cost containment.

In this report, we present findings from an analysis of impacts on patients' use of Medicare services other than demonstration home health during agencies' first two years of participation in the demonstration.[2] This analysis is important, because the goal of home health prospective payment is to reduce not only the cost of the Medicare home health benefit, but overall Medicare spending as well. Furthermore, increases in the use of and payment for Medicare-covered services other than home health care may be an indirect indicator of a decline in home health care quality resulting from prospective payment.

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[1]Strictly speaking, only for-profit agencies earn profits; nonprofit agencies generate surpluses that cannot be distributed to "owners" of the organization. However, for brevity, we use the term "profits" in this report to refer both to surpluses generated by nonprofit agencies and to profits earned by for-profit agencies.

[2]Trenholm (1998) describes findings of an analysis of demonstration impacts on home health care use. Originally, both reports were to focus on use during the entire three-year demonstration. However, Congress mandated that, beginning in 1999 (later changed to 2000), the Medicare program would use a prospective payment system when paying for home health services. In turn, HCFA requested that these reports be submitted before data from the third demonstration year would be available, so that HCFA could use the reports' findings in the design of the new payment system.

Section A of this chapter provides an overview of the Medicare home health benefit and the Medicare-certified home health industry. A reader familiar with Medicare home health may wish to proceed directly to Section B, which describes the Per-Episode Home Health Prospective Payment Demonstration. Section C describes the hypothesized effects of per-episode payment on Medicare services such as those provided by inpatient hospitals, skilled nursing facilities, and other Medicare providers. Section D provides a brief guide to the rest of this report and other evaluation reports.

A. THE MEDICARE HOME HEALTH BENEFIT

Congress established the Medicare home health care benefit in 1965, when the original Medicare program was created. Home health benefits were included to offer beneficiaries with acute conditions a less intensive and less expensive alternative to inpatient hospital care. The home health benefit has been modified at various times since the inception of the Medicare program, partly to increase access to care.

The current Medicare home health benefit covers home health services primarily under Part A, and deductibles and coinsurance do not apply.[3] To be eligible for home health benefits, a beneficiary must (1) have Medicare coverage; (2) be homebound; (3) be under the care of a physician; and (4) need skilled nursing, physical therapy, or speech therapy on a part-time or intermittent basis.[4]

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[3]In some rare instances, such as when an individual does not qualify as homebound or does not have Part A coverage, home health care may be covered under Medicare Part B. For Part B home health, deductibles and coinsurance do apply. In addition, under the Balanced Budget Act of 1997, the long-term use of home health services was transferred to Part B. However, our analysis period predates this change.

[4]Skilled nursing services are covered as long as (1) a physician has ordered them, (2) they are required on a part-time or intermittent basis, (3) they require the skills of a registered nurse (or of a licensed practical nurse or licensed vocational nurse under a registered nurse's supervision), and (4) they are reasonable and necessary to treat an illness or injury. Physical therapy and speech therapy are covered if a physician's assessment recommends these services. Beneficiaries who need (continued...)

Coverage under the home health benefit broadened considerably after the settlement of a lawsuit against HCFA in 1989; the broader coverage, in turn, contributed to dramatic growth in Medicare home health expenditures after that time. Medicare spending for home health care increased from $2.5 billion in 1989 to $16.8 billion in 1995, more than tripling the share of total Medicare outlays spent on home health (U.S. General Accounting Office 1998). Nearly all this growth has been the result of an increase in the provision of services, which has coincided with a dramatic expansion in industry size. In 1989, there were roughly 5,700 Medicare-certified home care agencies; by 1997, this figure had risen to more than 10,000.

As the use of home health services has grown nationwide, service use patterns have continued to differ strikingly across regions. For example, agencies provided an average of 47 visits during an episode of care to beneficiaries admitted to home health in 1990 and 1991, with the mean episode lasting 94 days. However, the average number of visits per episode varied from a low of 28 in the Pacific region to a high of 95 in the East South Central region, and the respective mean episode durations ranged from 60 days to 180 days (Schore 1995). By 1996, Medicare home health recipients received an average of 74 visits nationally, and the average varied from 48 in the Middle Atlantic and Pacific regions to 110 and 133 in the East and West South Central regions, respectively (Health Care Financing Administration 1998). By 1997, the average number of visits had fallen to 69 nationwide; the number ranged from a low of 30 visits in Washington State to a high of 153 visits in Louisiana (U.S. General Accounting Office 1998).

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[4](...continued)
only occupational therapy are entitled to benefits only if they have established a prior need for skilled nursing care, speech therapy, or physical therapy in the current or previous certification period (Teplitsky and Janson 1985-1992).

The dramatic growth in Medicare home health expenditures, combined with striking regional variation in service use and recent investigations of industry fraud and abuse, prompted Congress to legislate changes to the Medicare home health benefit as part of the Balanced Budget Act of 1997. The act mandated the implementation of a per-episode prospective payment system for Medicare home health in October 2000. Other changes to the home health benefit under the Balanced Budget Act included:

- Reducing the per-visit cost limit (known as the Section 223 Limits) from 112 percent of the mean to 105 percent of the median cost for freestanding agencies in the region. The Section 223 Limits were frozen for reporting periods that began between July 1, 1994, and June 30, 1996.
- Using a new algorithm to define the maximum payment for an agency. The algorithm is based on annual per-beneficiary costs or per-visit costs in a base year, whichever is lower. It is commonly referred to as the Interim Payment System (IPS), because it is intended to last only until the mandated prospective payment system takes effect.
- Eliminating coverage for blood drawing when it is the only home health service required
- Redefining "part-time" and "intermittent" care
- Redefining service location on the basis of the location of the patient rather than of the agency
- Requiring additional billing information (specifically, an identifier for admitting physician and visit duration)

The demonstration agencies were not subject to the changes in either the per-visit cost limit or the payment algorithm until after their participation in the demonstration ended. However, they were affected by the other changes.

B. THE PER-EPISODE DEMONSTRATION

Because a cost-reimbursed payment system does not provide a mechanism for home health agencies to earn profits by cutting costs, it offers no incentive for them to provide services

efficiently. Moreover, by reimbursing costs up to allowable limits, the system effectively subsidizes inefficient providers. The intent of per-episode prospective payment is to increase efficiency by using the profit motive as the primary incentive. Ninety-one Medicare-certified home health agencies in five states--California, Florida, Illinois, Massachusetts, and Texas--enrolled in the three-year per-episode demonstration.[5] Forty-seven of the agencies were randomly assigned to the treatment group and received per-episode payment. The remaining 44 were assigned to the control group and continued to operate under cost reimbursement. One control agency transferred into the treatment group near the start of the demonstration, leading to a revised total of 48 treatment agencies and 43 control agencies.[6] Each agency entered the demonstration and began implementing prospective payment at the start of its fiscal year. The first agencies in the treatment group began implementing prospective payment in June 1995; the latest entrants began in January 1996. Demonstration operations were planned to continue through December 1998. (The demonstration was extended for treatment group agencies until Medicare home health prospective payment is implemented nationally.)

Mathematica Policy Research, Inc. was the evaluation contractor responsible for the process and impact evaluations of the demonstration. Some of this analysis took place under a subcontract to the Veteran's Medical Research Foundation. Several other organizations also participated in the demonstration. Abt Associates, Inc. was the implementation contractor responsible for recruiting

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[5]Reflecting the United States more generally, the provision of Medicare home health varied considerably in the demonstration states. In 1997, the average visits provided per beneficiary were as follows: California, 47; Illinois, 47; Florida, 70; Massachusetts, 89; and Texas, 134 (U.S. General Accounting Office 1998). In a companion report, we will compare the demonstration agencies and agencies nationwide on home health provision.

[6]The transfer was made at the request of the agency that had established a network with two other agencies assigned to the treatment group. The three agencies planned to merge fully at the end of the demonstration.

demonstration agencies, monitoring the status of demonstration operations, and calculating certain statistics used to compute agency payments. Palmetto Government Benefits Administrator (PGBA) was the fiscal intermediary (FI) responsible for reviewing the claims of and payments to both treatment and control agencies. The Center for Health Policy Research (CHPR) at the University of Colorado was responsible for designing and implementing a quality assurance (QA) system for the demonstration agencies.

1. Demonstration Payment and Incentives

HCFA developed the Home Health Prospective Payment Demonstration to assess whether the profit motive can increase efficiency in providing Medicare home health care--and thereby reduce public expenditures--without sacrificing either access to care or the quality of care. Phase I of the demonstration, which tested per-visit prospective rate setting, gave agencies an opportunity to generate profits (and avoid losses) by reducing per-visit costs.[7] The evaluation of that demonstration found that some agencies earned profits, but they also did little to contain overall costs because there was no incentive to limit the services they provided. Phase II tested a per-episode prospective payment system. This system encourages agencies to reduce their per-visit costs and provides an additional incentive to reduce visits per patient episode. Thus, the opportunity to achieve cost savings was much greater under the Phase II payment system than under the Phase I system.

[7]Although the per-visit demonstration was implemented in the same five states as the per-episode demonstration, most of the agencies participating in the per-episode demonstration did not participate in the per-visit demonstration. (Only agencies in the per-visit control group were eligible to do so.) For details of the results of the per-visit demonstration, see Brown et al. (1995).

a. Payment

Agencies selected for the treatment group received a lump-sum payment for the first 120 days of home health care, regardless of the number or cost of visits provided.[8] The agencies were thus "at risk" for the costs of care incurred during this period. An agency could receive a new per-episode payment for a given Medicare beneficiary only after the 120-day at-risk period and a subsequent 45-day gap in services had elapsed. For each visit after the 120-day at-risk period that was not part of a new episode (referred to as "the outlier period"), treatment agencies received a fixed payment rate that varied by the type of visit. In the demonstration, a treatment agency was also paid on a per-visit basis for visits made to patients admitted before the agency began demonstration operations ("phase-in" visits) and to patients admitted within 120 days of the end of demonstration operations in that agency ("phase-out" visits).

Payments to agencies selected for the control group were based on the cost-based system in place at the start of the demonstration. Specifically, control agencies were reimbursed for their actual per-visit costs, up to 112 percent of the mean cost incurred by all agencies (for the agency's mix of visits) in the same geographic area.

b. Prospective Rate Setting

Prospective (per-episode) rates were based on a treatment agency's per-visit costs and episode profile during the fiscal year preceding its entry into the demonstration (the base year). The episode profile was determined by the average number of each of the six types of Medicare-covered visits

[8]Durable medical equipment, nonroutine medical supplies, and Part B home health services continued to be reimbursed at cost throughout the demonstration. In addition, if an agency did not provide one or more of the six Medicare services during the base year but began to do so during the demonstration, then those visits were also reimbursed at cost during the demonstration, as were the costs of care for which Medicare was a secondary payer.

that the agency provided during the first 120 days of the base-year episode. To obtain the per-episode payment for a demonstration year, the base-year episode profile used the agency's base-year per-visit costs adjusted for inflation. Payments for outlier, phase-in, and phase-out visits were based on the agency's base-year per-visit costs (adjusted for inflation).[9] HCFA's market basket was used to adjust both the per-visit and per-episode rates for inflation. Per-episode and per-visit payment rates were subject to HCFA's statutory home health cost limits.

The case-mix adjuster classified each patient into 1 of 18 groups on the basis of 12 variables that described the patient's characteristics. From this information, an aggregate case-mix index was created for each agency. At the end of each year of the demonstration, an agency's case-mix index for that year was compared with its case-mix index for the base quarter (the last quarter of the base year). If the agency's case mix differed, its aggregate payment was adjusted retrospectively.

c. Profit Sharing and Loss Sharing

To counteract the incentive to dramatically reduce services at the expense of quality, as well as to prevent agencies from realizing windfall profits at public expense, HCFA shared in any profits above selected levels. If the total of a treatment agency's per-episode and per-visit prospective payments was greater than the costs for services covered by the payments, any profit greater than five percent of total allowable costs for these services was subject to profit sharing. HCFA's share was one-quarter for profits ranging from 5 to 15 percent. Its share increased for profits of more than 15 percent by an amount that varied with the demonstration year.

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[9]Because complete data for episode profiles and settled cost reports were not available for a given year until some months after that year ended, the initial lump-sum and per-visit rates used in the demonstration were preliminary. They were revised after final base-year data became available.

To encourage agencies to participate in the demonstration, HCFA provided a loss-sharing arrangement. HCFA reimbursed treatment agencies for 99 percent of losses in the first demonstration year and for 98 and 97 percent of losses in the second and third demonstration years, respectively, as long as total payments were within the demonstration cost limits.

The demonstration cost limits were calculated as the sum of two subtotals: one for episodes and one for outlier, phase-in, and phase-out visits. The episode subtotal was obtained by costing out the agency's base-year episode profile at the cost limits for each demonstration year. The visit subtotal was obtained by costing out outlier, phase-in, and phase-out visits at the cost limits in that year.

2. Other Demonstration Procedures

Three other important demonstration procedures involved the methods for billing, the process of medical review, and the requirements for QA. Treatment agencies were strongly affected by all these demonstration procedures; control agencies were principally affected by the QA requirements. In this section, we briefly discuss each procedure.

a. Billing

To initiate an episode of care, treatment agencies had to submit an admission bill to PGBA. Treatment agencies also had to submit interim bills for services provided during the rest of the at-risk period, although they received payments only for supplies.[10] For services provided near the start of the outlier period, an agency had to "split" its standard bill into two bills--one for services provided up to the end of the at-risk period and one for services provided from the start of the outlier period.

[10]Agencies also had to submit interim bills to receive payment during outlier periods and to correctly calculate costs for profit or loss sharing with HCFA.

When a treatment agency patient was discharged during either the at-risk period or the outlier period, agencies had to submit a discharge bill to terminate the episode. PGBA would not initiate a new episode for a patient unless a previous episode had been terminated. In addition, before initiating a new episode, PGBA determined that the 120-day at-risk period and a 45-day gap had elapsed.

Periodic interim payments (PIPs), a means of "smoothing" cash flow for home health agencies, were originally discontinued for treatment agencies. However, a similar periodic payment system (termed "biweekly interim payments") was later introduced by PGBA to address cash flow problems in some agencies.

Control agencies continued to submit bills under cost reimbursement and continued to be eligible for PIPs. PGBA based the amount of the PIP on the agency's average cost for each type of visit. For some agencies, this method differed from the practices of their predemonstration FIs, which had based the PIP on overall agency average cost per visit. The new method may have led to minor differences in the PIPs that some control agencies received before the demonstration compared with those received after they entered the demonstration.

b. Medical Review

For agencies in the treatment group, PGBA conducted a limited medical review (known as an "abbreviated" medical review) of the care delivered during the at-risk period of the patient episode. This review attempted to determine whether the patient met the coverage criteria for home health care and whether the agency provided at least one visit that met these criteria. As a condition of payment, in addition to the admission bill, a treatment agency had to submit either (1) HCFA 485 and 486 forms (which contained information on the patient's health and eligibility status, as well as on the home health plan of treatment); or (2) clinical notes for the admission that coincided with an

episode eligible for prospective payment. The medical review was based on these materials. All services paid under per-visit rate setting (for example, those delivered during the outlier period) were subject to the usual medical review, under which PGBA reviewed a sample of claims to ensure that each visit was medically reasonable and necessary.

Payment of claims depended on the outcome of the medical review. If a treatment admission claim was accepted subsequent to abbreviated medical review, the per-episode payment was provided as a lump sum. If the admission claim was denied during the medical review, processing of subsequent claims for that episode was suspended for 65 days to await appeal. If an appeal was filed, these claims were further suspended until a decision on the appeal for the admission claim was made. If an admission claim was denied, but an appeal was not filed within 65 days, or if the denial of the admission claim was upheld on appeal, then subsequent claims were released for possible payment under the agency's per-visit rates.

Initially, all episodes were subject to abbreviated medical review; in May 1996, however, Medicare reduced the proportion to 25 percent. It made this change for two reasons: (1) the claims reviews took longer than expected, and (2) 100 percent medical review was considered unrealistic under a national program.

Medical review for control agencies continued under the nondemonstration regulations. For some control agencies, however, PGBA's review procedures may have differed from those of their predemonstration FIs, requiring them to adopt some minor procedural changes.

c. Quality Assurance

All demonstration agencies were required to collect and submit patient-specific information to CHPR, the demonstration QA contractor. The QA procedures used a continuous quality improvement approach. Visiting staff from demonstration agencies were required to collect

information (primarily on functional status and medical condition) at admission and at discharge, or 120 days after admission, whichever came first. Similar information was also collected before admission to an inpatient facility for stays of 48 hours or longer and after patients returned to home health care after these inpatient stays. CHPR used this information to develop profiles describing patient outcomes at each agency. CHPR then gave each demonstration agency its own profile to help it improve the quality of care it provided. The first profiles were sent to agencies in August 1997, just four months before the end of the period covered by this analysis.

C. HYPOTHESES CONCERNING THE EFFECT OF PER-EPISODE PAYMENT ON THE USE AND COST OF MEDICARE SERVICES

Per-episode payment provided home health agencies with a major financial incentive to change service provision. We expected per-episode payment to affect the use of other Medicare services through such changes. However, financial incentives were clearer during the 120-day at-risk period than for the rest of a home health episode. During the at-risk period, when agencies received a predetermined, lump-sum payment, agencies were expected to reduce the number of visits provided or change the mix of provided visits to a less costly combination. Agencies also had the incentive to reduce per-visit costs in other ways (for example, by reducing the length of visits, using less costly staff to provide visits, or reducing training or supervision levels or other administrative costs).

During the months following the at-risk period, the incentive to cut visits (or costs) was not as clear, and the strength of the incentive varied with agencies' predemonstration average costs. After the at-risk period, the agency received a predetermined payment for each visit, based on its average predemonstration cost. If the payment was at or above its costs, the agency had an incentive to increase the number of visits, rather than to lower them. If this payment was less than the agency's per-visit costs, however, it had an incentive to reduce the number of visits to limit losses (although

this incentive might have been offset by the desire to maintain volume). Visits would also be reduced in the post-at-risk period if earlier discharges led to fewer patients still receiving services after 120 days.

The hypothesis that agencies would reduce visits overall was strongly supported by the analysis of the impact of per-episode payment on home health use. That analysis found that agencies reduced the number of visits provided by about 17 percent (from 45 to 37 visits) during the risk period and by about 33 percent (from 30 to 20 visits) during the eight months following the risk period. These reductions were basically uniform across all types of visits, so agencies did not change the visit mix substantially. The analysis also found that agencies receiving per-episode payment reduced episode length by as much as 33 days, or nearly 25 percent (Trenholm 1998).

However, a preliminary analysis suggested that per-visit costs increased by about eight percent during the risk period. Increased costs were largely the result of the resources required to decrease service use. For example, increased supervision was one method agencies used to reduce the number of visits provided. This increased supervision, however, directly increased per-visit costs (Cheh and Trenholm 1999).

Reductions in either the number of home health visits provided or the cost per visit might result in poorer care quality and patient outcomes (such as declines in functional ability or health) and, thus, increased use of other Medicare services. If home health visits are reduced past a certain point, patients are likely to suffer outcomes adverse enough to be reflected in the increased use of other services, particularly inpatient hospital admissions and emergency room use. It was unclear, however, what level of visit reduction would have these consequences. Alternatively, reductions in home health visits may not affect care quality but may nevertheless result in increased use of other

services that may substitute for home health care (such as skilled nursing facilities, hospices, outpatient facilities, and physicians' offices).

In particular, earlier discharge from demonstration agencies might cause patients to receive more services from home health agencies other than the admitting demonstration agency. Earlier discharge, however, may reflect either inadequate or more efficient home health care. If demonstration home health agencies inappropriately curtail visits to patients who need them and another agency provides the needed care, the use of home health services from an agency other than the admitting demonstration agency reflects poor care on the part of the admitting agency. On the other hand, if admitting agencies eliminate discretionary visits at the end of an episode, but the patient has become accustomed to receiving home care and feels the need for additional visits, another home health agency might be persuaded to provide them. In a highly competitive home health environment, this might not take much "persuading," even for a patient only marginally qualified for home health care (or not qualified at all). In this case, the receipt of services from another home health agency reflects additional unnecessary costs to the Medicare program. However, a preliminary analysis of the effect of per-episode payment on Medicare service use and reimbursement during the at-risk period suggested that substantial reductions in the number of home health visits provided had no effect on the use of other home health or Medicare-covered services during that period (Schore 1998).

D. GUIDE TO THE REST OF THIS REPORT AND OTHER EVALUATION REPORTS

The next chapter of this report describes the data sources, samples, and variables used to examine Medicare service use under the demonstration. Chapter III describes the statistical methods used to estimate demonstration impacts. Chapter IV presents findings from an analysis of demonstration impacts on Medicare service use and reimbursement during the first year after patient

admission (that is, the 120-day at-risk period and the two four-month periods that follow). Chapter V presents demonstration impacts for specific agency and patient subgroups, and Chapter VI summarizes the key findings and presents our conclusions.

This report was part of a series developed for the evaluation of the Per-Episode Home Health Prospective Payment Demonstration. Other reports include descriptions of analyses of prospective payment's impact on home health use (Trenholm 1998), care quality (Chen 1999), and non-Medicare service use (Phillips 1999). Future reports will describe demonstration implementation and agencies' selection and retention of patients.

II. DATA

Our study of Medicare service use is based on a data file that contains 119,358 observations corresponding to episodes of demonstration home health care beginning in the 91 participating agencies between their demonstration start and August 31, 1997. The file is built primarily from Medicare claims data extracted from the Health Care Financing Administration's (HCFA's) Standard Analytic Files in May 1998. Allowing a four-month window for the files to include all paid claims, our file should contain complete service use data through December 31, 1997.[1]

Because agencies entered the demonstration at the start of their 1996 fiscal years, rather than all at the same time, the period the file covers varies by agency. A majority of the agencies (57) entered the demonstration on January 1, 1996, enabling us to identify 20 months of demonstration admissions with at least 4 months of postadmission data for them. The remaining 34 agencies enrolled earlier--between June 1, 1995, and November 1, 1995--allowing us to identify 21 to 27 months of demonstration admissions for those agencies.

From this main file, we created an analysis sample of 94,357 patient observations in 87 agencies. We excluded some observations from the main file for one of two reasons. First, we excluded all the records for three demonstration agencies that had left the demonstration and one

[1]Although almost all claims should be in the Standard Analytic Files within the four months, a small number of demonstration agencies experienced significant delays in billing that may have caused the file to undercount the number of visits provided. These agencies were dropped from the analysis of impacts on the use of demonstration home health services. (See Trenholm [1998] for further information on this topic.) However, the agencies were included in this analysis because of its focus on Medicare services *other than* demonstration home health. A comparison of Medicare service use and reimbursement impact estimates using samples with and without agencies that had billing problems suggested their inclusion had no effect on estimates.

agency that had too few admissions to provide reliable data. Second, we excluded a number of individual patient observations because some data were missing or invalid.

In the next two sections of this chapter, we provide further details on the construction of the main file and our approach to developing an analysis sample. In the last section, we describe the analysis sample in terms of the mean values of control variables that will be used in the impact analysis to account for preexisting differences between the treatment and control groups.

A. CONSTRUCTING THE MAIN DATA FILE

Each record in the main file includes the following information: (1) the patient's use of Medicare-covered services for up to a year following home health admission,[2] (2) patient characteristics at admission, (3) the patient's Medicare service use from the six months preceding admission, (4) the predemonstration characteristics of the demonstration home health agency, and (5) the characteristics of the county in which the agency is located. These data came primarily from Medicare databases; a limited number of variables came from other sources. The Medicare Standard Analytic Files contained claims data that were the basis of measures of Medicare service use and reimbursement during periods preceding and following demonstration home health admission. The Medicare Enrollment Database provided basic demographic information on patients, including their race/ethnicity, gender, and age. Demonstration data collected for case-mix adjustment and for monitoring quality of care provided more detailed information on patient health status at admission. In addition, predemonstration, patient-level, case-mix and claims data were aggregated to the agency level to construct a measure of agency "practice pattern." Agency cost reports for the base year and

[2]Because our service use data are complete only through December 31, 1997, the most severely truncated records are for patients with home health admissions occurring on our cutoff date (August 31, 1997). For these patients, we have service use data during only the first four months after admission (that is, only for the at-risk period).

county-level data from the Area Resource File (ARF) provided the remaining data required to construct agency- and area-level measures.

1. Identifying Individual Patient Episodes

For patients in both the treatment and control agencies, we used data from UB-92 bill record files obtained from the demonstration fiscal intermediary (FI) to identify home health episodes as defined by demonstration rules.[3] We scanned the UB-92 files, beginning the file search from the date of each agency's enrollment in the demonstration, to identify the first admission for an individual and all of that person's subsequent bill records. The first demonstration admission determined the episode start date from which we constructed the first record for the patient. To determine the end of the initial episode and the start of any subsequent episode, we tracked bill records until we observed a 45-day gap in care that began after the end of the at-risk period (that is, after the first 120 days from admission). This procedure was followed regardless of whether the agency discharged and readmitted a patient within the first 165 (120 + 45) days after the initial admission.

If we observed a readmission for a patient after 165 days, and a 45-day gap in care had taken place, we created a second record for the patient corresponding to this second demonstration episode. We then repeated the process until we had constructed, for each patient, a series of records for all the episodes beginning in the agency between its demonstration start date and August 31, 1997.

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[3]We used the UB-92 bill records rather than HCFA claims data principally because the former contain data on patient characteristics at admission. These characteristics data were then used to construct several key control variables.

2. Creating Outcome Variables

To construct outcome variables, we used claims data from the Medicare Standard Analytic Files. After matching patients with home health episodes identified from UB-92 bill record data to the Standard Analytic Files, we extracted all claims for a given patient during the 12 months after the episode start. Both Part A and Part B claims data were extracted.

The outcome variables used in this analysis measure the use of and reimbursement for the following services: emergency room, inpatient hospital, skilled nursing facility, hospice, nondemonstration home health, outpatient hospital, physician and other practitioner, durable medical equipment, and other Medicare Part B services. (See Table II.1.) In this analysis, we describe demonstration impacts on the use of home health care delivered only by agencies *other than demonstration agencies;* impacts on the use and cost of demonstration home health care are the subject of other evaluations reports.[4]

For most outcome measures, three categories of variables were constructed:

1. ***Any Use of Services.*** Indicator variables equal to one if services were used, and zero otherwise
2. ***Intensity of Service Use.*** Variables signifying the number admissions, visits, or days
3. ***Reimbursement.*** Continuous variables describing the cost of the service to the Medicare program

[4]This analysis also describes impacts on the use of home health services delivered by demonstration agencies under Medicare Part B, for those patients who also had Part A coverage. Under demonstration procedures, the only Part B home health claims that fall under a per-episode payment are those for *patients with no Part A coverage* who received home health care usually covered by Part A.

TABLE II.1

OUTCOME VARIABLES DESCRIBING MEDICARE-COVERED SERVICE USE AND REIMBURSEMENT DURING THE YEAR FOLLOWING DEMONSTRATION EPISODE START

Inpatient Hospital Services
- Any admission
- Number of admissions
- Number of days
- Reimbursement[b]

Emergency Room Services
- Any emergency room encounter resulting in admission
- Number of emergency room encounters resulting in admission
- Any emergency room encounter not resulting in admission
- Number of emergency room encounters not resulting in admission
- Any emergency room encounter
- Total number of emergency room encounters

Skilled Nursing Facility Services
- Any admission
- Number of admissions
- Number of days
- Reimbursement[b]

Hospice Services
- Any admission
- Number of days
- Reimbursement[b]

Demonstration Home Health Part B Services[h]
- Number of visits
- Reimbursement

Nondemonstration Home Health Services[a]
- Any admission
- Number of visits
- Reimbursement[b]

Outpatient Hospital Services[c]
- Any services
- Reimbursement

Physician and Other Practitioners
- Any visits
- Reimbursement

Durable Medical Equipment
- Any purchases[d]
- Reimbursement

Other Part B Services[e]
- Any use
- Reimbursement

Total Reimbursement
- Part A[f]
- Part B
- Total[g]

SOURCE: Medicare Standard Analytic Files.

[a]Home health services provided by an agency other than the treatment or control agency that originally provided the identifying home health admission.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for services under Medicare Part B.

[c]Includes both emergency and nonemergency visits to outpatient hospital facilities, as well as use of laboratory and radiology services.

[d]Includes the purchase or rental of equipment, as well as its repair, maintenance, and delivery.

[e]Includes diagnostic laboratory and radiology services (including pathologist and radiologist services) from nonhospital providers, supplies and devices, mammography, ambulance, covered medications, blood, and vaccines.

[f]Excludes reimbursement for home health services provided by demonstration agencies. Includes reimbursement for inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Medicare Parts A and B.

[g]Excludes reimbursement for home health services provided by demonstration agencies.

[h]Includes only demonstration agency services reimbursed under Medicare Part B that are not considered under prospective payment, namely ambulatory home health services for patients who have Part A coverage. For example, patients who (1) had been receiving therapy visits under a home health plan of treatment but were no longer homebound and wanted to continue with the same therapists in an outpatient setting, or (2) were in skilled nursing facilities but had exceeded their lifetime skilled nursing facility coverage limits and received therapy services under Part B home health.

3. Creating Control Variables

As discussed in Chapter III, the impact analysis relies on regression models that control for possible preexisting differences between treatment and control agencies and their patients. The control variables used in the regression models, shown in Table II.2, fall into one of two general categories: (1) patient characteristics, including basic health and demographic descriptions at admission, Medicare service use during the six months preceding admission, and (for patients for whom quality monitoring data were available) measures of health and functioning at admission; and (2) characteristics of the admitting agency and the county in which the agency is located.

a. Patient Characteristics

Patient characteristics at the start of an episode and Medicare service use preceding the episode controlled for differences between the patients of treatment and control agencies. We expect that individuals who are more severely ill, have diagnoses requiring greater amounts of care, and have greater limitations in daily living activities may require more Medicare home health and other services.

We obtained data on patient characteristics at the start of the home health episode from four sources: (1) the UB-92 Medicare billing form, which includes data collected for the demonstration's case-mix adjuster; (2) the Medicare Enrollment Database; (3) the Medicare Standard Analytic Files; and (4) data collected by the demonstration quality assurance (QA) contractor at home health admission. In the remarks field of the admission UB-92 bill, all agencies were required to submit information on patient characteristics necessary to create the 18-category Home Health Utilization Group case-mix adjuster. These characteristics include measures of impairment in activities of daily living, as well as whether the patient has certain medical conditions (cancer, diabetes, stroke,

TABLE II.2

CONTROL VARIABLES FOR MULTIVARIATE ANALYSIS, BY SOURCE

Patient Level			Agency Level	Area Level
Patient Characteristics at Episode Start	Medicare Service Use Preceding Episode (Medicare Standard Analytic Files)	Patient Characteristics at Episode Start (QA Start of Care Data)	Agency Characteristics	County Characteristics (Area Resource File)
UB-92 Case-Mix Data	Length of inpatient stay during two weeks before home health	Health risk factors	Base-Year Cost Reports	Physicians per 10,000 (1994)
Has cancer	Whether in SNF during two weeks before episode start	Urinary tract infection	Hospital-based	Nursing home beds per 100 elderly residents (1991)
Has diabetes	Total Part A Medicare reimbursement in six months prior to episode start	Incontinent or used catheter	Profit status	Hospital occupancy rate (1993)
Had stroke	Calendar quarter of episode start	Change in medical regimen	Agency size	Mean Medicare reimbursement per beneficiary (1991)
Has decubiti stage 3 or 4		Clinical stability	State	
Needs complex wound care		Life expectancy	Costs relative to limits	
Has limitations in bathing, eating, dressing, toileting, transferring		Medical prognosis	Implementation Contractor	
Admitted to home health from hospital		Functional prognosis	Urban/rural indicator	
Medicare Enrollment Database		Does not walk independently	Chain member	
Age		Unable to prepare light meals	Base-Quarter Demonstration Case-Mix File	
Gender		Unable to take oral medications	Agency practice pattern index	
Race		Unable to perform light housekeeping		
Original reason for entitlement		Has shortness of breath		
Had Medicare for less than six months		Is confused		
Had Medicaid buy-in for Part A or B Medicare		Is depressed		
In HMO for some time during past six months		Has emotional difficulties		
Medicare was second payer for some time during past six months		Has cognitive difficulties		
		Has live-in informal assistance		

TABLE II.2 *(continued)*

Patient Level			Agency Level	Area Level
Patient Characteristics at Episode Start	Medicare Service Use Preceding Episode (Medicare Standard Analytic Files)	Patient Characteristics at Episode Start (QA Start of Care Data)	Agency Characteristics	County Characteristics (Area Resource File)
		Has live-in paid help		
		Likely to be able to take over care		
		Has nonhealing surgical wound		

NOTE: Control variables in regression models for estimating the impact of Per-Episode Payment on the use and cost of Medicare-covered services also included a lagged value of each dependent variable, measured over the six months prior to home health episode start and based on data from the Medicare Standard Analytic Files.

HMO = health maintenance organization; QA = quality assurance; SNF = skilled nursing facility.

decubiti) and care needs (complex wound care). Medicare enrollment files provided us with basic patient demographic information, including the patient's age (at the start of home health episode), gender, race/ethnicity, and disability status (from the original reason for Medicare qualification), whether the patient had Medicare coverage for the full six months prior to home health admission (the period over which lagged dependent variables were constructed), and whether the patient had been in a health maintenance organization (HMO) or had a primary payer other than Medicare during the six months prior to admission.

The Standard Analytic Files provided data to construct measures of Medicare service use and reimbursement, measures that served both as proxies for health status or severity of illness at the time of home health admission and as predemonstration values of dependent variables (sometimes referred to as lagged dependent variables). Severity of illness was reflected in measures of the length of a hospital stay ending during the two weeks before home health admission (which was zero for patients who had not been in the hospital during that period), whether the patient had been in a skilled nursing facility during those two weeks, and total Medicare Part A reimbursement during the six months before home health admission.[5]

[5]For patients with incomplete data for that period, we assigned the sample mean and created a set of three indicator variables to identify the reason for any imputation. The indicator variables identified (1) patients in an HMO during the six-month preadmission period; (2) patients for whom Medicare was a second payer during that period; and (3) patients younger than age 65.5, who generally were not enrolled in Medicare for a full six months. For this last group, we assigned the mean values for those aged 65.5 to 66 years, rather than the mean for the entire sample. Only 2.3 percent of patients in the analysis sample had incomplete data for any of these reasons. Imputing means for a small number of observations with missing explanatory variables in this way is standard practice. The indicator variables account for differences that may exist between those observations for whom the explanatory variables are available and those observations for whom they are not. Dropping the observations with missing explanatory variables from the analysis would jeopardize its generalizability.

Lagged dependent variables are the best predictors of use and reimbursement during the periods following home health admission and account for differences between treatment and control agency patients in the use of Medicare services before the start of demonstration home health episodes. Lagged variables for this analysis comprised measures of Medicare service use and reimbursement, by type of service, during the six months before the home health episode. (Table II.1 describes lagged variables as well as outcome measures.)

In a set of limited analyses designed to investigate the robustness of our results, we used an additional set of control variables that contain more detailed information on patients. These variables, shown in the third column of Table II.2, were constructed from the demonstration QA data. We used these variables in a limited capacity because they were available for only about 35 percent of the patients in our sample.[6]

Selected QA variables were also used to define three pairs of patient-level variables used for subgroup analysis: (1) patients who can or cannot take oral medications independently, (2) patients who do or do not currently have paid care or live-in informal care, and (3) patients whose expected home health costs are relatively high or relatively low. The last subgroup was constructed using a number of QA variables identifying the agency's planned treatments. These treatment variables were (1) administration of insulin, (2) administration of other intramuscular or subcutaneous injections, (3) management and evaluation of patient care plan, (4) treatment for difficulty

[6]QA data for much of the sample were missing for two reasons. First, agencies did not collect these data until May 1996, as much as one year into the demonstration for some agencies. Second, agencies varied in their degree of compliance in submitting these data to the QA contractor. Chen and Noveck (1998) provide additional discussion of the QA procedures and data.

swallowing, (5) partial/complete bedbath, and (6) personal care.[7] The remaining variables used to construct the expected cost subgroup were obtained from the remarks field of the UB-92, as well as from the QA form. They identify whether the patient had diabetes, decubiti (stage 3 or stage 4), or a need for complex wound care.

b. Agency Characteristics

We constructed a number of agency-level control variables because different types of agencies might have varying abilities or incentives to reduce visits in response to the demonstration. In addition, certain types of agencies might serve a mix of patients requiring more (or less) care than the patients of other agencies. For example, proprietary and nonprofit agencies might have different preexisting practice patterns with respect to the number of visits rendered per episode, and, relative to freestanding agencies, hospital-based agencies might serve patients who are somewhat more acutely ill.

The agency-level variables are based on data from base-year Medicare cost reports and from the demonstration implementation contractor. The cost reports supplied data on the agencies' state, profit status, and affiliation; they also enabled us to characterize agency size, as measured by the total number of visits provided during the base year.[8] Data collected by the implementation contractor before the demonstration started provided information on whether an agency was a member of a chain or was located in a rural area (as defined by the U.S. Bureau of the Census).

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[7]Because the planned treatments may be partly determined by whether an agency is in the treatment or control group, indicators of planned treatments may be endogenous to patient outcomes and, thus, were not included among the set of additional control variables in the QA-enhanced regression models.

[8]We defined small agencies as those providing fewer than 30,000 visits during the base year (approximately 25 percent of the sample) and large agencies as those providing 30,000 visits or more.

The agency practice-pattern variable is an index of the average number of visits per episode provided by an agency during the first 120 days of base-quarter episodes relative to the average number provided by other demonstration agencies. The index accounts for differences across agencies in the average number of visits, by discipline, and in the characteristics of their patients. A value greater than one indicates that, controlling for differences in case mix, an agency provided more visits during the 120-day period than did other demonstration agencies during the quarter preceding the demonstration.

The specific construction is as follows. Let subscript *i* refer to the service type and subscript *j* refer to case-mix cell. Using the case-mix adjuster (developed by HCFA and the implementation contractor), we classify an agency's patients into 1 of 18 case-mix cells ($j = 1,...,18$). Within each case-mix cell, we multiply the average visits of each type for a given agency (n_{ij}) by its national cost limit in the base year (w_i), and then sum across the visit types. This sum essentially reflects a weighted count of the average visits for an agency within a case-mix cell. We then use these weighted counts to construct the ratio of the agency's average number of visits received by patients in the case-mix cell ($\Sigma w_i n_{ij}$) to the average among all agencies for this cell ($\Sigma w_i N_{ij}$). Finally, for each agency, we arrive at the practice-pattern index by summing across the 18 case-mix ratios, weighting each ratio by the agency's proportion of episodes in the case-mix cell during the base quarter (p_j). Thus, for each agency, the index practice pattern is given by:

$$(1)\quad \sum_j p_j \left(\frac{\sum_i w_i n_{ij}}{\sum_i w_i N_{ij}} \right).$$

Selected agency-level control variables were also used to define the following four pairs of variables used for subgroup analysis: (1) practice pattern (low use or high use), (2) profit status (for profit or nonprofit), (3) size (small or large), and (4) auspice (freestanding or hospital based). In addition, we used cost report data submitted for the base year to define a fifth agency-level subgroup: whether an agency was above or below its base-year cost limits.

c. Area Characteristics

We also constructed control variables to account for differences in county-level characteristics that might influence the care an agency renders. For example, in areas where the number of nursing home beds is limited (relative to demand), hospitals may discharge to home health care patients who otherwise would be discharged to a nursing home. We obtained county-level characteristics from the ARF, including the number of physicians per 10,000 residents, the number of nursing home beds per 100 elderly residents, and hospital occupancy rate. We also obtained mean Medicare reimbursement per beneficiary to capture county-level differences in overall practice patterns and health care costs.

B. FORMING THE ANALYSIS SAMPLE

As noted, the main file of 119,358 observations contained a number of records that could not (or should not) be part of the sample used to analyze demonstration impacts on Medicare service use and reimbursement. The records excluded from the analysis sample are of two types. The first includes *all* admissions to three demonstration agencies that left the demonstration near its start and to one agency that had too few admissions to provide reliable estimates of its patients' service use in a typical year. The second type pertains to patients who were HMO members or had Medicare as

a second payer on or after their admission date, had missing information on outcome or key control variables, or already had a previous demonstration admission.

1. Excluding Agencies

We dropped all patients from the analysis sample for two control agencies and one treatment agency that left the demonstration near its start. These agencies either were purchased by or merged with another agency, and the new ownership did not want to be part of the demonstration. By dropping these three agencies, we excluded 2,293 patients from the analysis sample. We also dropped all patients from one agency that had just 19 admissions--too few to provide reliable estimates of its patients' service use in a typical year.

After these agency-level exclusions, we were left with 87 agencies, 46 in the treatment group and 41 in the control group. The 87 agencies had 117,046 admissions between their demonstration start date and August 31, 1997, or 98 percent of the total for the 91 originally randomized agencies.

2. Excluding Patients

We also dropped a number of individual patient records. We dropped 5,608 records because HCFA's enrollment files showed that the patients had been enrolled in Medicare HMOs on or after the episode start date. It would have been inappropriate to include these records because home health services provided to managed care patients are not subject to prospective payment (or to traditional cost reimbursement). We dropped another 3,166 records because Medicare was a secondary payer for the patients on or after the episode start date. As with managed care patients, the home health services provided to these patients are not subject to per-episode payment, so agencies' behavior should be largely independent of the demonstration incentives.

We dropped another 4,432 records because critical data items were missing. For approximately a third of the records, the data fields of their UB-92 forms did not contain complete information on patient characteristics.[9] Another third of the 4,432 records did not show any home health services during the first month after admission. In most cases, these records reflected "false" admissions, in which the only service provided was durable medical equipment. The rest of the 4,432 records were excluded because one or more of the Medicare use or reimbursement variables constructed for this analysis were less than zero, indicating in most cases that some claims in a series of bills and adjusted bills were missing from the Standard Analytic Files.

We also excluded patients from the analysis sample if our file already contained an admission from a previous demonstration episode. In total, 9,483 records were dropped for this reason. As discussed in Chapter III, these subsequent admissions were dropped because they could have led to incorrect conclusions about the impacts of prospective payment on service use.[10] In general, much or all of the care identified on these records had already been identified on the initial record for the patient.

3. Analysis Samples

After dropping all these records from the main file, we arrived at an analysis sample that contained 94,357 unique patient admissions to 87 demonstration agencies (46 in the treatment group and 41 in the control group). This sample reflects 79 percent of the admissions in our main base file.

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[9]This information had been inadvertently erased from the forms near the start of the demonstration because of an error in the software used by the demonstration FI. Although we used data from the QA admission forms in an attempt to replace the missing information, only a small number of records could be repaired, because the QA forms were not collected until May 1996.

[10]The basic concern is that prospective payment provides an incentive for treatment agencies to distribute care over multiple episodes and obtain multiple episodic payments for the same patient. This incentive could lead to bias in our impact estimates unless we were to count service use over a fixed period from the start of the *initial* admission.

As Table II.3 shows, a majority of the records in the analysis sample (57,008--25,809 for control agency patients and 31,199 for treatment agency patients) pertain to patients admitted during an agency's first year in the demonstration. A little more than half the records (54 percent) are for treatment agencies. All records for the agencies' first year contain 12 months of information on service use subsequent to the episode start. Most of the remaining records (35,834) reflect admissions in the second demonstration year. Most of these year 2 records provide eight months of data on postadmission service use, but only 6,805 records contain 12 months of data. The analysis sample contains only a small number of records for demonstration year 3. These records have information for only the first 120 days of care (the at-risk period), and they include just 18 demonstration agencies.

Because the patient records provide varying amounts of postadmission data, we typically must restrict the sample that we use for a given analysis. (Chapter III discusses this issue fully.) For example, in much of our main impact analysis presented in Chapter IV and in our subgroup analysis presented in Chapter V, we examine Medicare service use and reimbursement over a period of 12 months after a demonstration home health admission. As Table II.3 shows, however, we have 12 months of complete data for all agencies for only the first demonstration year. Given this data constraint, we restrict our main analysis sample to the 57,008 patients with demonstration home health admissions in the first demonstration year.

C. PATIENTS AND AGENCIES IN THE MAIN ANALYSIS SAMPLE

Patients and agencies in our main analysis sample differed significantly by treatment status on a number of characteristics, which underscores the importance of controlling statistically for these differences in our impact estimates. When the number of units randomized is not large, as is the case

TABLE II.3

NUMBER OF PATIENTS IN THE ANALYSIS SAMPLE COVERING SELECTED TIME PERIODS, BY DEMONSTRATION YEAR AND TREATMENT STATUS
(Number of Agencies in Sample in Parentheses)

Number of Patients with Complete Data from Demonstration Admission Through:	Total		Year 1		Year 2		Year 3	
	Control Agency Patients	Treatment Agency Patients	Control Agency Patients	Treatment Agency Patients	Control Agency Patients	Treatment Agency Patients	Control Agency Patients	Treatment Agency Patients
Four Months (120 Days)	44,559 (41)	49,798 (46)	25,809 (41)	31,199 (46)	17,659 (41)	18,175 (46)	1,091 (12)	424 (6)
Eight Months (240 Days)	38,200 (41)	42,588 (46)	25,809 (41)	31,199 (46)	12,391 (41)	11,389 (46)	--	--
One Year (365 Days)	30,447 (41)	33,366 (46)	25,809 (41)	31,199 (46)	4,638[a] (20)	2,167[a] (16)	--	--

SOURCE: Medicare Standard Analytic Files and demonstration UB-92 file.

[a]Excludes admissions on January 1, 1997.

serve those who had recently been discharged from a hospital. Thus, treatment agencies were less likely to serve patients with pressing posthospital care needs but more likely to serve patients who had already received a spell of nursing care or rehabilitation in a nursing home. However, these differences are not large and may be due in part to there being a higher percentage of hospital-based agencies among the control group (as we discuss later).

Lagged Medicare Service Use and Reimbursement. Regression models also included values of dependent variables measured over the six months before home health admission. (These are often referred to as lagged dependent variables. Their means, by treatment status, appear in Table II.5.) Statistically significant differences between patients of treatment and control agencies in hospital and nursing home use are similar to those described above for the two weeks before home health admission. Other statistically significant differences were relatively small (for example, differences in the receipt of home health care from nondemonstration agencies and in the use of emergency room, physician, and outpatient services).

In general, the means paint a picture of beneficiaries with a high level of service use in the months leading up to home health admission, as would be expected for beneficiaries sick and impaired enough to qualify for the Medicare home health benefit. Nearly two-thirds were hospitalized at some time during the six months before home health (even though only about a third had been admitted to home health directly from the hospital). More than a fifth had spent some time in a skilled nursing facility, and roughly 15 percent had received home health care from a nondemonstration agency during the six months. (About eight percent had received home health from a demonstration agency during the period; this figure is not in the table.) Use of emergency room services was also high: just over a third had an emergency room visit that resulted in an inpatient admission, while a quarter had visits that did not. More than two-thirds use outpatient

TABLE II.5

WEIGHTED MEANS FOR LAGGED DEPENDENT VARIABLES, BY TREATMENT STATUS FOR DEMONSTRATION YEAR 1
(Measured Over the Six Months Before Demonstration Home Health Admission)

	Treatment Group	Control Group
Inpatient Hospital Services		
Any admission (percent)	61.0	64.0***
Number of admissions	.94	1.02***
Number of days	8.2	8.9***
Reimbursement[a] (in dollars)	8,554	8,397
Emergency Room Services		
Any emergency room encounter resulting in admission (percent)	37.4	38.8*
Number of emergency room encounters resulting in admission	.48	.50**
Any emergency room encounter not resulting in admission (percent)	25.1	25.4
Number of emergency room encounters not resulting in admission	.39	.38
Any emergency room encounter (percent)	51.9	53.5**
Total number of emergency room encounters	.86	.89
Skilled Nursing Facility Services		
Any admission (percent)	22.3	20.9**
Number of admissions	.28	.27*
Number of days	6.2	5.9
Reimbursement[a] (in dollars)	1,934	1,936
Hospice Services		
Any admission (percent)	.2	.2
Number of days	.3	.2
Reimbursement[a] (in dollars)	26	16
Demonstration Home Health Part B Services[b]		
Any visits (percent)	.2	.2
Number of visits	.04	.11*
Reimbursement (in dollars)	3	6

TABLE II.5 *(continued)*

	Treatment Group	Control Group
Nondemonstration Home Health Services[c]		
Any admission (percent)	15.4	14.2**
Number of visits	8.81	8.45
Reimbursement[a] (in dollars)	604	567
Outpatient Hospital Services[d]		
Any services (percent)	67.5	69.0**
Reimbursement (in dollars)	678	680
Physician and Other Practitioners		
Any visits (percent)	93.5	95.2***
Reimbursement (in dollars)	1,829	1,893*
Durable Medical Equipment		
Any purchases (percent)	41.7	41.4
Reimbursement (in dollars)	274	251
Other Part B Services[e]		
Any use (percent)	89.2	91.0***
Reimbursement (in dollars)	549	529
Total Reimbursement		
Part A[f] (in dollars)	11,103	10,897
Part B (in dollars)	3,330	3,354
Total[g] (in dollars)	14,647	14,542
Number of Patients	**31,199**	**25,809**

SOURCE: Medicare Standard Analytic Files.

[a]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for services under Medicare Part B.

[b]Includes only demonstration agency services reimbursed under Medicare Part B that are not considered under prospective payment, namely, ambulatory home health services for patients who have Part A coverage. For example, for patients who (1) had been receiving therapy visits under a home health plan of treatment but were no longer homebound and wanted to continue with the same therapists in an outpatient setting, or (2) were in skilled nursing facilities but had exceeded their lifetime skilled nursing facility coverage limits and received therapy services under Part B home health.

TABLE II.5 *(continued)*

[c]Home health services provided by an agency other than the treatment or control agency that originally provided the identifying home health admission.

[d]Includes both emergency and nonemergency visits to outpatient hospital facilities, as well as use of laboratory and radiology services.

[e]Includes diagnostic laboratory and radiology services (including pathologist and radiologist services) from nonhospital providers, supplies and devices, mammography, ambulance, covered medications, blood, and vaccines.

[f]Excludes reimbursement for home health services provided by demonstration agencies. Includes reimbursement for inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Medicare Parts A and B.

[g]Excludes reimbursement for home health services provided by demonstration agencies. Is the sum of Part A and Part B reimbursement.

*Difference between treatment and control groups significantly different from zero at the .10 level, two-tailed test.
**Difference between treatment and control groups significantly different from zero at the .05 level, two-tailed test.
***Difference between treatment and control groups significantly different from zero at the .01 level, two-tailed test.

services such as diagnostic laboratory or radiology. More than 40 percent had durable medical equipment purchases. Almost all had seen their physicians or other practitioners, and 90 percent had reimbursement for "other" Part B services (which are dominated by diagnostic laboratory and radiology provided outside a hospital). As a result, mean Medicare reimbursement for the *six months* prior to home health admission was just under $15,000 for patients in our analysis sample, compared with an *annual* average of roughly $5,000 for all beneficiaries in 1996 (Health Care Financing Administration 1998).

2. Patient Characteristics for the Quality Assurance Subsample

Start of Care data from the demonstration's QA system were available for about a third of the patients who comprise the main analysis sample for estimating impacts on Medicare use and reimbursement during demonstration year 1. (There were QA data for all 87 agencies in that sample, however.) The QA database contained some measures of patient functioning, and treatment-control differences on these measures for QA subsample usually were consistent with the functioning differences for the larger analysis sample. (See Table II.6.) For example, treatment agency patients were somewhat better able than control patients to prepare light meals and manage oral medications on their own. Among control agency patients, 64 percent were unable to prepare light meals and 57 percent were unable to manage their medications, compared with 59 and 54 percent of treatment patients, respectively. Slightly lower proportions of treatment agency patients were clinically unstable (52 versus 56 percent) and had more favorable prognoses, both medically (86 versus 83 percent) and functionally (68 versus 64 percent). Treatment agency patients also appeared to be in better mental health than control patients: they were less likely to be depressed (21 versus 26 percent for control agency patients) or to have emotional or cognitive difficulties (22 versus 27 percent and 24 versus 29 percent, respectively). Finally, treatment patients were assessed as being more likely

to be able to take over treatment following home health or were more likely to have helpers who could do so (71 versus 65 percent).

3. Agency and Area Characteristics for the Main Analysis Sample

There are several significant differences in the characteristics of treatment and control agencies. (See Table II.7.) While the proportion of treatment group patients from agencies (48 percent) is significantly less than the control proportion (51 percent), this difference is not large. On the other hand, we do observe large and statistically significant differences in the proportions of treatment and control group patients from agencies that are hospital-based, chain members, or relatively small in terms of visits provided in the base year. Only 9 percent of treatment group patients were served by hospital-based agencies, compared with 15 percent of control patients. Thirty-seven percent of treatment group patients were served by agencies that were chain members, compared with 27 percent of control patients. Small agencies (that is, agencies that provided fewer than 30,000 visits in their base year), served 35 percent of treatment group patients, compared with 20 percent of control patients, a difference of 75 percent.

We also observe a statistically significant difference between treatment and control agency patients in agency practice patterns prior to the demonstration. On average, the practice pattern index is about 15 percent lower for treatment than control agencies, suggesting that, even before the demonstration, treatment agencies provided fewer visits than control agencies to roughly similar patients.

The distribution of treatment and control agency patients across states differs, with a higher prevalence of treatment agency patients in Massachusetts (17 percent, compared with 7 percent of control patients), but a lower prevalence in Illinois (13 percent, compared with 22 percent of control patients). Significant differences also existed between the service environments of the counties in

TABLE II.6

WEIGHTED MEANS FOR SUPPLEMENTAL PATIENT-SPECIFIC CONTROL VARIABLES FROM QA DATA, BY TREATMENT STATUS FOR DEMONSTRATION YEAR 1
(Percentages, Unless Otherwise Noted)

	Treatment Group	Control Group
Behavioral Risk Factors		
Heavy smoker, obese, alcoholic, or drug dependent	19.2	22.0***
Engages in none of the above	71.5	71.8
Unknown whether any risk factors	9.3	6.2***
Urinary Tract Infection		
Treated for infection in past 14 days	8.9	9.2
Not treated in past 14 days (or on prophylactic treatment)	8.7	8.8
Unknown whether any treatment in past 14 days	3.8	3.0*
Incontinent or Requires Catheter	24.1	25.3
Has Nonhealing Surgical Wound	1.1	1.7**
Medical Regimen Changed in Past 14 Days	65.5	61.0***
Very or Somewhat Clinically Unstable	52.2	56.3***
Overall Prognosis for Recovery Good or Fair	86.4	83.0***
Life Expectancy Less than Six Months	9.6	6.8***
Rehabilitative Prognosis Good	67.6	64.3***
Ambulates with Human or Mechanical Assistance or Is Chair- or Bedfast	20.3	18.6*
Unable to Prepare Light Meals or Reheat Delivered Meals	58.6	63.5***
Performs Light Housekeeping with Assistance/Supervision or Is Unable to Perform	66.6	68.0
Able to Take Oral Medications only with Assistance or Unable to Take Oral Medications Unless Administered by Someone Else	54.0	57.3***

TABLE II.6 *(continued)*

	Treatment Group	Control Group
Has Shortness of Breath with Moderate or Minimal Exertion or When at Rest	38.9	42.0***
Is Confused upon Awakening, at Night Only, or Throughout the Day, or Is Nonresponsive	13.2	14.5
Has Depressive Feelings[a]	21.0	26.4***
Has Emotional Difficulties[b]	21.7	27.1***
Has Cognitive Difficulties[c]	23.6	28.5***
Has Live-in Unpaid Help	44.4	45.8
Has Paid Help or Is in Assisted-Living Facility	16.7	16.7
Very or Somewhat Likely That Patient or Helpers Can Take over Treatment	71.4	64.9***
Number of Patients	**13,040**	**6,501**

SOURCE: Quality Assurance Start of Care instrument.

[a]Patient feels helpless, a sense of failure, or hopeless, or is preoccupied with death, or has thoughts of suicide.

[b]Patient has crying spells, withdraws from social interaction, has sleep disturbance, is unwilling to become more independent, is agitated, or has attempted suicide.

[c]Patient has memory deficit, has impaired decision making, is verbally disruptive, is physically aggressive, is socially inappropriate, or is delusional.

*Difference between treatment and control groups significantly different from zero at the .10 level, two-tailed test.

**Difference between treatment and control groups significantly different from zero at the .05 level, two-tailed test.

***Difference between treatment and control groups significantly different from zero at the .01 level, two-tailed test.

TABLE II.7

WEIGHTED MEANS FOR STANDARD AGENCY- AND AREA-SPECIFIC CONTROL VARIABLES, BY TREATMENT STATUS FOR DEMONSTRATION YEAR 1
(Percentages, Unless Otherwise Noted)

	Treatment Group	Control Group
Predemonstration Ratio of Mean Agency Visits to Mean for All Demonstration Agencies (Case-Mix Adjusted)	.93	1.10***
Hospital-Based Agency	8.7	14.6***
For-Profit Agency	47.8	51.2***
Chain Member	37.0	26.8***
Agency Provided Fewer than 30,000 Visits in Base Year	34.8	19.5***
Agency Located in Urban Area	84.8	85.4
State		
Florida	8.7	9.8**
Illinois	13.0	22.0***
Massachusetts	17.4	7.3***
Texas	34.8	39.0***
County-Level Means		
Number of nursing home beds per 100 persons over age 65	5.1	5.2**
Number of physicians per 10,000 persons	22.0	21.5***
Hospital occupancy rate	62.3	60.8***
Medicare reimbursement per beneficiary (in thousands of dollars)	3.4	3.4
Number of Patients	**31,199**	**25,809**

SOURCE: Demonstration Case-Mix File; Medicare Cost Reports; Area Resource File.

*Difference between treatment and control groups significantly different from zero at the .10 level, two-tailed test.

**Difference between treatment and control groups significantly different from zero at the .05 level, two-tailed test.

***Difference between treatment and control groups significantly different from zero at the .01 level, two-tailed test.

which treatment and control agencies were located. There were significant differences in nursing home bed and physician supplies and hospital occupancy rates; however, these differences were small.

4. Implications for the Analysis of Program Impacts

In summary, while the preexisting treatment-control differences in patient characteristics were minimal, several large differences exist in agency characteristics and in the distribution of agencies across states. If these differences affect the provision of Medicare-covered services, we would incorrectly estimate the effects of per-episode payment if we were simply to compare treatment and control group means. In fact, the magnitude of these differences suggests that a comparison of treatment and control group means could be misleading. Because regression models allow us to control for these preexisting differences, they ought to provide more accurate estimates of the effect of the demonstration payment method.

III. METHODS

Our analysis focuses on how per-episode prospective payment for home health affects the provision and cost of other Medicare-covered services relative to a payment system of cost reimbursement. To conduct this analysis, we must address two key methodological issues. First, we must properly define the reference period for Medicare service outcomes. Second, we must use appropriate statistical techniques to estimate demonstration impacts.

A. REFERENCE PERIODS

Our hypotheses regarding the effect of per-episode prospective payment on Medicare service use and reimbursement stem from our expectations that home health agencies will alter patterns of service provision during and following the at-risk period. We therefore define reference periods for service use and reimbursement outcomes to be consistent with those for demonstration home health use outcomes during an episode of care. A complete patient episode under the demonstration includes (at most) an initial 120-day ("at-risk") period for which agencies receive a single prospective (episodic) payment, followed by an "outlier" period for which they receive prospectively set per-visit payments. This episode forms a natural unit of analysis, as it encompasses all the care an agency provides for a given spell of illness. As discussed below, because an agency's treatment status may affect the length and number of episodes over which care is distributed, an impact analysis based on the patient episode could lead to misleading estimates. To avoid this complication, we will examine demonstration impacts over fixed periods that occur after an initial home health admission. Such fixed periods enable us to estimate unbiased measures of program impacts because the demonstration incentives cannot affect them.

1. Why Investigating Impacts Over a Patient Episode Is Problematic

If treatment agencies respond to the demonstration by reducing episode length, using an episode of care as the reference period could overstate demonstration impacts. For example, consider a patient who is admitted to a control agency and receives home health services for three months. If this patient were admitted to a treatment agency, the incentives of the demonstration might lead that agency to provide services for just two months. Suppose that both patients are hospitalized three times during the three months following home health admission, once each month. The control agency patient would then have three inpatient admissions during the home health episode, but the treatment agency patient would have only two. If this pattern were typical for the treatment and control groups, and the reference period was the home health episode, one might erroneously conclude that treatment group agencies reduced inpatient admissions by a third, even though treatment and control agency patients had identical numbers of admissions over the three-month period. This example underscores the importance of focusing on service use measures during fixed time periods that are independent of the system of payment.

2. The "Patient-Year" as Primary Reference Period

To avoid the problem associated with using episodes of care as the unit of analysis, we focus on patients' service use during a fixed period after home health admission. In principle, this period could be any length; however, we face a trade-off between increasing duration and losing observations because of incomplete data. After taking this trade-off into account, we decided to use a "patient-year"--the first 365 days following home health admission--as the period for our main analysis.

To more completely understand how and when impacts may be taking place, we also examine impacts within (1) the first four months (120 days) of the patient-year--that is, the at-risk period; (2)

the subsequent four months of the patient-year (from day 121 through day 240); and (3) the final four months of the patient-year (from day 241 through day 365).

We have the greatest number of patient-year observations for agencies that entered the demonstration earliest and the fewest observations for agencies that entered the latest. For example, because Medicare data for this analysis were complete through December 1997, agencies that entered the demonstration the earliest (in June 1995) had a full year of post-home health admission data for patients admitted between June 1995 and December 1996 (that is, patients admitted during the first 19 months of demonstration operations). By contrast, agencies that entered the demonstration last (in January 1996) only had a full year of data for patients admitted between January and December 1996 (the first 12 months of demonstration operations). The benefit of using *all* patients for whom a year of post-home health admission data is available is that they would improve the precision of our impact estimates. However, because agencies' behavior may be related to how long they have participated in the demonstration, our estimates should be based on a consistent period of participation for all agencies. Thus, for our main analysis, we limited our sample to patients with admissions that started during agencies' first 12 months in the demonstration.[1]

Because we are interested in whether demonstration impacts change as agencies gain more experience with the demonstration, we also compare impacts during the agencies' first and second years in the demonstration. The start date of each episode is used to determine the demonstration year in which the observation falls. If the start date occurs after the 365th day since the agency

[1]Because we are using patients, rather than episodes, as the unit of analysis, we define patient years only using the first admission we observe for a given patient and ignore any admissions resulting from subsequent home health episodes. By doing so, our impact estimates are unaffected by the possible correlation between treatment status and the likelihood of a subsequent home health episode, and we avoid double-counting services.

entered the demonstration, it falls within the second demonstration year; otherwise, it falls within the first.[2] This leads to the creation of two separate samples, the first corresponding to admissions that begin in demonstration year 1 and the second to those that begin in demonstration year 2.

For most "late" agencies (those that entered the demonstration on January 1, 1996), we had a full 12 months of postadmission data on only a few patients admitted during the second demonstration year; for some of those agencies, we had data on no patients (see Figure III.1).[3] The comparison of impacts between demonstration years must therefore be limited to periods of care shorter than a patient-year. The two periods that we examine are (1) the first 120 days after home health admission (the at-risk period), and (2) the first eight months after admission. To achieve the highest possible precision in our estimates, each of these analyses uses all of the (initial) admissions for which we have sufficiently complete service data.[4] For the analysis of the at-risk period, we have between 8 and 12 months of admissions for all agencies in the second demonstration year; for the analysis of the first 8 months, we have between 4 and 8 months.

B. STATISTICAL TECHNIQUES

We use regression models to estimate demonstration impacts because they control for preexisting differences between treatment and control agencies that may exist despite random assignment and because they improve statistical precision. For our main findings, we estimate

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[2]For six agencies that entered the demonstration before August 31, 1995, we have a small number of admissions in demonstration year 3 for which we observe service use for at least 120 days postadmission. We will not investigate these data because there are too few observations to enable us to draw valid conclusions.

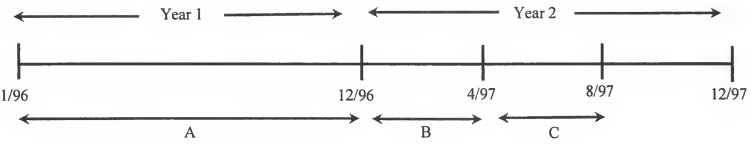
[3]Specifically, we have one complete patient-year of data for 228 patients admitted to 35 (of the 45) late agencies on the agencies' first day in the demonstration (January 1, 1996).

[4]We are less concerned about consistently defining the samples for demonstration year 2 because our goal is simply to identify changes in impacts between years. To the extent that we find any changes taking place, we can further evaluate how they would differ if we defined the agency samples over equal periods from the start of the demonstration.

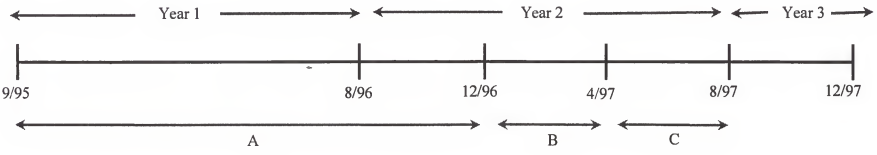
FIGURE III.1

AVAILABILITY OF DATA TO COMPARE IMPACTS BETWEEN DEMONSTRATION YEARS

Agencies Beginning the Demonstration "Late," on January 1, 1996



Agencies Beginning the Demonstration "Early," Before January 1, 1996[a]



For admission occurring in interval:

A = Full 12 months (patient-year) of postadmission data available.

B = Less than 12 months of postadmission data available. All admissions complete through at least 8 months.

C = Less than 8 months of postadmission data available. All admissions complete through at least 4 months.

[a]Early agencies began the demonstration between June 1, 1995, and November 1, 1995. The agency start date shown (September 1, 1995) is for illustrative purposes.

impacts using logistic regression when the dependent variable is binary and ordinary least squares (OLS) regression otherwise. As part of the sensitivity analysis, we estimate impacts using Tobit regression models when the dependent variable is "censored"--that is, has a natural (or imposed) lower (or upper) bound at which a portion of the sample is clustered. (Reimbursement for skilled nursing facility services, for example, is censored at zero because most people in our sample were not admitted to a skilled nursing facility.) We also estimate impacts using Poisson models when the dependent variable is a count variable (for example, number of inpatient admissions). The main regression models use weighted data that give each agency equal representation in the analysis; however, as a sensitivity test, we also examine impacts in the absence of sample weights. All analyses use standard errors that take into account the effects of sample clustering and weighting.

In this section, we describe our approach to (1) estimating overall impacts, (2) assessing hypothesis test results, (3) estimating subgroup impacts, (4) comparing overall impact estimates for demonstration years 1 and 2, (5) weighting observations, (6) accounting for the clustering of observations within agencies, and (7) assessing the robustness of overall impact estimates.

1. Overall Impact Estimates

When the outcome that we investigate is continuous (for example, total Medicare reimbursement), the basic model that we use to estimate the overall impacts of prospective payment is:

(1) $Y = \alpha + X\beta + \delta T + \epsilon,$

where:

Y = a continuous outcome variable, measured after a patient's admission to a demonstration home health agency

X = a matrix of control variables

T = a binary variable for treatment status that equals one for admissions to treatment agencies and zero for admissions to control agencies

α = the intercept term

β = the vector of regression coefficients on the control variables

δ = the regression coefficient on the variable for treatment status

ϵ = a random disturbance term assumed to have a mean of zero (conditional on the regressors in the model) that reflects all the unobserved factors affecting the outcome variable

The coefficient δ on the variable T measures the effect of prospective payment on the (continuous) outcome of interest and is tested to determine whether it is significantly different from zero.

When the outcome variable that we investigate is binary (for example, whether a patient had an inpatient admission), we used the logit model to estimate demonstration impacts. The structure of the logit model is as follows:

$$(2) \quad Probability\ (Y = 1) = \frac{1}{1 + e^{(\alpha + X\beta + \delta T)}},$$

where Y is a binary outcome variable and the remaining variables and parameters are defined as in equation (1).[5]

Given the nonlinearity of the logit model, the estimated impact of prospective payment is not measured directly by the coefficient δ on the variable for treatment status. Instead, to estimate the

[5]For simplicity, all later specifications in which the outcome variable may be continuous or binary are shown for the continuous outcomes only. In all cases, however, we use both OLS and logit models as appropriate.

demonstration impact on the probability that $Y = 1$, we use the coefficient estimates from the model to generate two predicted probabilities for each observation: one assuming that the observation belongs to the treatment group ($T = 1$), and one assuming that it belongs to the control group ($T = 0$). The impact estimate is the average difference between these estimated probabilities. Because the statistical significance of δ determines whether the treatment-control difference in the odds that $Y = 1$ is significantly different from zero, we use the p-value on this parameter to test our hypotheses about differences between the two groups.

Overall impact estimates are presented in Chapter IV. Throughout the tables of results, along with impact estimates and corresponding p-values, we present the mean of each outcome variable for the control group as a reference point. The control group mean is weighted, but not regression-adjusted, and reflects a reasonable estimate of the mean value for a particular outcome that could be expected to occur in the absence of the demonstration.

2. Hypothesis Tests

For each outcome, a two-tailed t-statistic tests the null hypothesis that there is no difference between the regression-adjusted population means for treatment and control agencies.[6] The associated p-value, which indicates the probability of obtaining a sample estimate of the observed magnitude if the null hypothesis were true, is used to determine whether the demonstration had a measurable impact. The p-value is based on estimated standard errors that account for the clustering of patients within agencies and the use of sample weights. A p-value of less than .10 indicates rejection of the null hypothesis and provides significant statistical evidence that a demonstration

[6]Two-tailed tests are used throughout our analysis to avoid confusion and to flag statistically significant estimates that suggest findings that run counter to expectations. For impacts with the "correct" expected sign, a two-tailed test is less likely than a one-tailed test to reject the hypothesis of no demonstration effect (all else equal).

impact probably exists. At this p-value, however, approximately 10 percent of independent tests will show, simply by chance, a statistically significant treatment-control difference when there is no true program effect (known as a Type I error). Therefore, in assessing whether a statistically significant treatment-control difference, especially one with a p-value between .05 and .10, should be interpreted as a true program impact, we consider whether the sign and magnitude of the predicted effect are consistent with those for related outcomes.

Despite our large sample of patient admissions, it is unlikely that small treatment-control differences will be statistically significant, because design effects greatly reduce the precision of our estimates. For example, if we ignore the design effects associated with our weighted sample and the clustering of patients within agencies, we have an 80 percent probability of detecting an impact on Medicare Part A reimbursement of 4 percent under a two-tailed test at the 10 percent significance level. After accounting for design effects, however, this minimum detectable effect is approximately 8 percent. Thus, despite our large sample size, it is unlikely that we would detect the effects of prospective payment on outcomes unless they were at least moderate. Nonetheless, in most cases, the sample is adequate to detect impacts of policy-relevant size.

3. Subgroup Impacts

We investigate demonstration impacts on key outcomes for subgroups of observations for two reasons. First, if impacts for the full sample exist, we are interested in determining whether they differ between key subgroups of agencies or patients. Second, even if we find no compelling evidence of overall impacts, it is important to determine whether countervailing impacts exist for selected subgroups. Key outcomes for the analysis of Medicare service use and reimbursement refer to the following variables measured over the year following a demonstration home health admission: number of inpatient admissions, number of emergency room encounters, whether had a skilled

nursing facility admission, whether had a nondemonstration home health admission, Medicare Part A reimbursement, and Medicare Part B reimbursement.

The subgroup analysis uses a regression model similar to equation (1) or (2). The only difference is that the subgroup model includes additional control variables formed by interacting treatment status T with a set of binary (subgroup) variables defined from agency or patient characteristics. For the agency subgroup analysis, we estimate a single equation with a set of five subgroup variables interacted with treatment status. Each subgroup variable included in the regression reflects a key agency characteristic (for example, agency is for-profit), and its corresponding (reference) group reflects the absence of this characteristic (for example, agency is nonprofit).[7] For the patient subgroup analysis, we estimate a separate subgroup regression for each individual characteristic.[8]

The basic regression model for subgroups is given by:

$$(3)\quad Y = \alpha + X_1\beta_1 + X_2\beta_2 + \delta T + \sum_{i=1}^{n} \gamma_i (X_{1i} * T) + \epsilon,$$

where:

Y = an outcome variable

[7]The agency characteristics and their respective subgroup pairs are (1) practice pattern--high-use or low-use, (2) profit status--for-profit or nonprofit, (3) size--small or large, (4) auspice--freestanding or hospital-based, and (5) costs--above or below the base-year limits.

[8]The patient characteristics and their respective subgroup pairs are (1) management of oral medications--patient can or cannot take independently, (2) caregiver availability--patient has or does not have paid or live-in informal care, and (3) expected home health costs--patient has high or low expected costs. The possibility that, in a pooled model, the impacts by high/low expected costs will obscure other subgroup impacts lead us to estimate each of these subgroup models individually. Although impacts may be similarly obscured in a pooled agency subgroup model, it is desirable in this case to isolate the agency characteristics through which prospective payment operates.

X_1 = a matrix of n subgroup variables, each of which takes on a value of one if the observation belongs to the subgroup and zero otherwise[9]

X_2 = a matrix containing all other control variables

T = a binary variable for treatment status that equals one for admissions to treatment agencies and zero for admissions to control agencies

β_1 and β_2 = vectors of regression coefficients on the variables in X_1 and X_2, respectively

γ_i = a regression coefficient on the interaction term between X_{1i} and T[10]

α = the intercept term

δ = a regression coefficient on the variable for treatment status

ϵ = a random disturbance term assumed to have a mean of zero (conditional on the regressors in the model) that reflects all the unobserved factors affecting the outcome variable

To assess whether the impacts between particular subgroups differ, we examine the statistical significance of the coefficient γ_i on the corresponding interaction term. For example, suppose that the p-value for the coefficient on the interaction term between an agency's profit status and its treatment status is .04. This test indicates that the effect of prospective payment on that outcome measure differs significantly between for-profit and nonprofit agencies.

We also estimate the effect of prospective payment for each individual subgroup.[11] The impact for a subgroup is estimated by setting the indicator variable for the subgroup appropriately and then

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[9]For the (pooled) agency subgroup model, n equals five; for each (separate) patient subgroup model, n equals one.

[10]This interaction term takes on a value of one if the observation belongs to the ith subgroup *and* is a treatment admission, and zero otherwise.

[11]It may not be possible to identify small or moderate impacts for subgroups, as tests of statistical significance lose power as the sample size decreases. The smaller the size of the subgroup (all else equal), the less likely we are to reject the hypothesis that the payment method has no effect, for any given true effect size.

obtaining the predicted value of the outcome variable for each observation, first as if it were from a treatment agency ($T = 1$) and then as if from a control agency ($T = 0$). The mean difference between these two predicted values provides an unbiased estimate of the impact of prospective payment on the outcome Y for each subgroup. Thus, for the subgroup of agencies/patients with characteristic j, the impact estimate is given by:[12]

$$(4) \quad \delta + \sum_{i \neq j} \gamma_i \overline{X_{1i}} + \gamma_j X_j,$$

where $\overline{X_{1i}}$ equals the sample mean of the ith subgroup variable X_{1i}.

To assess whether the impact for a particular subgroup is different from zero, we examine the statistical significance of the expression in equation (4). For example, suppose that the subgroup was for-profit agencies and that the p-value for that expression was .04. This test indicates that prospective payment had a significant effect on the outcome measure for patients of for-profit agencies.

Subgroup impact estimates for key outcomes are discussed in Chapter V. Appendix B contains detailed subgroup impact tables, including p-values for impact estimates within subgroups (as estimated in equation [4]) and of differences between impact estimates across subgroups (as estimated in equation [3]).

4. Differences in Impacts Between Demonstration Years

To examine whether the impact of prospective payment changes with agencies' length of participation, we combine the available sample from demonstration year 2 with our main analysis

[12]For the patient-level subgroup analysis, the expression simplifies to: $\delta + \gamma_j X_j$.

sample from demonstration year 1 and estimate a single regression model for key outcomes. The specification of this model is similar to those shown in equations (1) and (2) for continuous and binary outcomes, respectively. The only difference is that the specification includes an additional binary variable for the demonstration year in which the admission took place and an interaction term between the agency's treatment status and the demonstration year. That is:

$$(5) \quad Y = \alpha + X\beta + \delta T + \lambda_1 D + \lambda_2 (T * D) + \epsilon,$$

where:

- Y = an outcome variable
- D = a dummy variable that equals one if the admission took place in demonstration year 2, and zero if it is from demonstration year 1
- X = a matrix of control variables
- T = a binary variable for treatment status that equals one for admissions to treatment agencies and zero for admissions to control agencies
- λ_1 = the coefficient on the dummy variable D
- λ_2 = the coefficient on the interaction term between the variables D and T[13]
- α = the intercept term
- β = the vector of regression coefficients on the control variables
- δ = a regression coefficient on the variable for treatment status
- ϵ = a random disturbance term assumed to have a mean of zero (conditional on the regressors in the model) that reflects all the unobserved factors affecting the outcome variable

[13]This interaction term equals one for admissions in the second demonstration year to treatment agencies, and zero otherwise.

As before, the estimated impact of prospective payment in the first demonstration year is given by the coefficient δ. The estimated impact in the second demonstration year is given by the sum of coefficients δ and λ_2. The difference between these two impacts, λ_2, then measures the change in the effect of prospective payment from the first to second year of the demonstration.

We found no substantial differences in impact estimates for key outcomes between samples based on year 1 and year 2 demonstration home health admissions. (The results of these statistical comparisons are not described further in this report. However, Appendix A contains impact estimates for year 2 admissions.)

5. Weighting

We weight observations in *all* models to give each agency equal representation in the analysis. We use this approach for two reasons. First, because the demonstration is implemented at the agency level (not the patient level), the agency is the behavioral unit of interest. Second, the use of weighted data ensures that the experiences of a few large agencies will not dominate the impact estimates. Preventing this dominance is important because the largest agencies have more than 20 times the number of admissions as do the smallest agencies.

For each patient in agency i, we construct the "agency-equal" weight (w_i) as the ratio of the average number of patient-level observations per agency (n/k) to the number in the ith agency (n_i):

$$(6) \quad w_i = \frac{n/k}{n_i},$$

where n is the total number of observations for all agencies and k is the number of agencies. Thus, patient observations for larger than average agencies are given less weight and those for small agencies are given greater weight. For the comparison of impacts between demonstration years, we

use a separate agency-equal weight in each year. Thus, if an agency is larger (relative to other agencies) in the second year of the demonstration, the weight for observations (patients) in this agency would have a smaller value in the second year than in the first.

6. Design Effects

To draw appropriate inferences about the expected effects of a national prospective payment program, our estimated standard errors must reflect the fact that our observations are clustered in a relatively small number of agencies. The variances of the impact estimates generated from standard statistical packages account for the number of patients included in the sample, treating them as though they are a random sample from an infinite population of patients.[14] However, they do not account for the fact that our sample consists of patients from a limited number of agencies.

To correct for possible nonindependence of observations within agencies, we use SUDAAN software to obtain the appropriate standard errors for OLS and logit impact estimates and STATA software for Tobit and Poisson impact estimates. The SUDAAN and STATA calculations also account for the greater variance introduced by using sample weights in the regression models.

7. Robustness of Impact Estimates

Although we expect the regression models used in the main analysis to be robust, they may be sensitive to four important factors. First, the use of sample weights that equate agencies' representation in the data might give small agencies undue influence in the analysis. Second, agencies with atypically high or low average values of outcome variables might have undue

[14]Our sample actually includes the population of (initial) patient admissions taking place in demonstration agencies over the early demonstration period. However, because we wish to make inferences about the outcomes for patients admitted at other times and to other agencies, we treat patients in the data as though they were drawn in a simple random sample from the pool of all (future) patients in all agencies.

influence on the analysis. Third, unmeasured characteristics of agencies or their patients might introduce bias into our impact estimates. Finally, our OLS models are not strictly appropriate for censored or count outcome measures and might yield biased estimates of program effects on these outcomes. We conducted tests of the sensitivity of key findings to these four factors (these tests are described next) and found that, in general, our results were quite robust. (Some of these tests are discussed further in Chapter IV, but their results do not appear in tables.)

a. Unweighted Estimates

To investigate the sensitivity of our results to the weighting approach, we reestimated the impacts of prospective payment on key outcomes without agency weights. If impact estimates are similar under the two approaches, then it is very likely that our results are not overly influenced by a small number of anomalous observations from agencies with few admissions. Thus, the results may be more broadly interpreted for policy purposes. Conversely, although dissimilar results under the two sample weights do not necessarily indicate that the main results are "incorrect," they do suggest the need for additional assessment to determine the most valid inferences for policy.

We found no substantial differences between weighted and unweighted impact estimates for key outcomes except as noted in Chapter IV.

b. Outliers

The distributions of most of our outcome variables are not highly skewed. However, it is still important to examine the degree to which a small number of outlier observations affect impacts on service use and reimbursement, particularly for agencies whose patients have unusually high or low mean values for outcome variables. To investigate the effect of such observations on impact estimates, we reestimated models excluding all admissions from eight agencies--with the two highest

and two lowest mean values for a given outcome in both the control group and the treatment group. Because we used the sample weights that give each agency equal weight, the removal of these 8 outlier agencies left about 91 percent (79 of the 87 agencies) of the weighted sample intact.

We found no substantial differences in impact estimates for key outcomes between models that exclude outlier agency patients and those that include them, except as noted in Chapter IV.

c. Unmeasured Differences Between Agencies and Their Patients

Given the large treatment-control differences found in selected agency characteristics (shown in Table II.7), a natural question is whether other, unmeasured differences between treatment and control agencies could lead to bias in our impact estimates, because our regressions do not control for these differences. To address this question, we included the additional set of patient-level control variables available from the quality assurance (QA) admissions data and reestimated the impacts of the demonstration for key outcomes.[15]

The addition of the QA variables did not change the interpretation of our impact estimates. They did reduce the size of a few statistically significant estimates somewhat, however, suggesting that some potentially relevant patient characteristics were missing from our basic models.

d. Censored Outcome Variables

The Tobit model is used to obtain consistent estimates of impacts on outcomes where the dependent variable has been censored at a particular value.[16] By comparing the results from these

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[15]The QA variables, summarized in Table II.2, were not used in the main regression models because they were available for only a subset of patients.

[16]The consistency of the Tobit model rests on strong statistical assumptions, and violation of these assumptions may lead to substantial bias in the estimated impacts. Therefore, we prefer to use this model only as a sensitivity test and to rely on more robust OLS models for the main findings.

models with those obtained from OLS regressions, we determined the potential sensitivity of our main findings to the effects of censoring.

Variables may be censored from the left, right, or both sides. However, in this analysis we only have left-censored variables. All variables for reimbursement for a particular service (or unit of service) are censored on the left at a value of zero for patients who did not use that service.

The Tobit model for our left-censored dependent variables assumes that an unobserved, underlying index of service use or reimbursement exists. If this index (Y^*) exceeds some threshold (zero in our specification), then the patient receives services; if the index is less than the threshold, the patient receives no service. The model therefore consists of a probabilistic component for the likelihood of requiring a particular type of service and a linear component for the expected amount of service or level of reimbursement (conditional on having a need for services above the threshold):

$$(7) \qquad Y^* = \alpha + X\beta + \delta T + \epsilon$$
$$Y = Y^* \text{ if } Y^* > 0$$
$$Y = 0 \text{ if } Y^* \leq 0,$$

where Y^* is the underlying index variable that determines the value of the outcome Y, and the remaining variables and parameters are defined as in equation (1).

After the parameters of the Tobit model have been estimated, they are used to obtain predictions of the expected value of the given outcome for each observation. This expected value is given by the product of the probability that a patient uses a given service and the amount of the service used or reimbursement received (conditional on it being greater than zero). These predicted values are calculated twice: once assigning a given observation to the control group and once assigning it to the treatment group. The mean difference between these two predicted values is our estimate of the impact of the demonstration on a given outcome.

We estimated Tobit models for the outcomes inpatient reimbursement and skilled nursing home reimbursement during the year following demonstration home health admission (for year 1 admissions). Tobit impact estimates did not differ appreciably from OLS estimates. OLS estimates are presented in tables in Chapter IV.

e. Count Outcome Variables

Poisson models predict the expected number of events occurring within a given interval of time. The model is Pr $(Y=i) = \exp(-e^{a+Xb+dT})\, e^{(a+Xb+dT)i} / i!$, where i is the number of events occurring, X is the row vector of control variables, treatment status is represented by the binary variable T, and b and d are the estimated parameters. The expected value of Y is $e^{a+Xb+dT}$, so the proportionate effect of prospective payment on the expected number of admissions or encounters is measured by e^{d}, for which a confidence interval is constructed.

We estimated Poisson models for the outcomes number of inpatient admissions and number of emergency room encounters during the year following demonstration home health admission (for year 1 admissions). We found similar estimates for Poisson and OLS models; regression estimates are presented in tables in Chapter IV.

IV. OVERALL IMPACTS ON THE USE OF AND REIMBURSEMENT FOR MEDICARE-COVERED SERVICES

Our analysis of the impact of per-episode payment on the provision of home health services suggests that per-episode payment reduced the number of home health visits provided during the year after admission by roughly a quarter, from an average of 76 visits to just 58 visits (Trenholm 1998). Reductions in home health visits (as well as reductions in costs) could have reduced care quality or caused a shift in care to other settings. Figure IV.1 presents an overview of how per-episode payment might, as a result of these reductions, increase the use of and reimbursement for other Medicare-covered services. Nonetheless, we find no evidence that per-episode payment increased such use or reimbursement. This lack of increased use of other services suggests that a reduction in home health care at the level observed under the demonstration does not adversely affect care quality or lead to cost shifting.

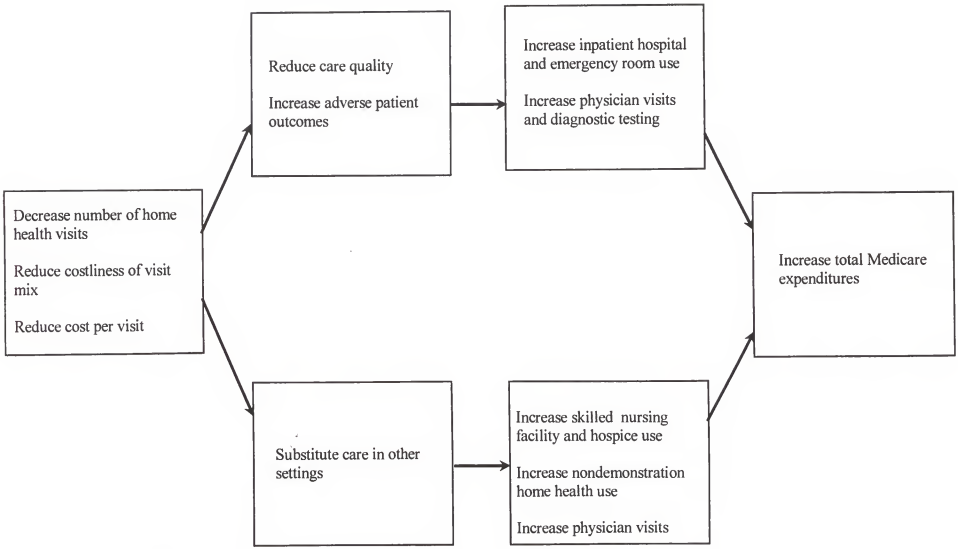
A. EMERGENCY ROOM AND INPATIENT SERVICES

A key concern about the implementation of per-episode payment was that the quality of home health care might suffer as a result of financial incentives to reduce the number of visits provided or to reduce the cost of providing visits. The most serious adverse effects on quality would be reflected in increases in emergency room visits and inpatient admissions. The findings of this analysis suggest, however, that the quality of care was not affected in this way.

During the year following home health admission, just over 35 percent of all patients had an emergency room visit that resulted in an inpatient admission, while just under 35 percent had an emergency room visit without an admission. The number of total emergency room contacts during

FIGURE IV.1

HOW PER-EPISODE PAYMENT MIGHT INCREASE OVERALL MEDICARE SPENDING



the year--including both those that did and did not lead to inpatient admissions--was approximately 1.3, on average.[1] (See Tables IV.1 and A.1 for year 1 and year 2 admissions, respectively.)[2]

Treatment group patients had somewhat lower levels of emergency room use than control patients during each four-month period following home health admission. These treatment-control differences usually were small, but they were consistent across outcomes, and most were statistically significant at the 10 percent level or better (although, among year 2 admissions, fewer differences were statistically significant, primarily because the estimated differences were smaller). Moreover, models using alternative weighting schemes and statistical procedures (not presented in tables) suggested that these small reductions in emergency room use were reasonably robust. Models reestimated without weighting each agency equally yielded difference estimates that were all negative, but smaller and with only one significant at the 10 percent level. (These reductions in the size of the difference estimates and their loss of statistical significance were due primarily to a few small treatment group agencies having atypically low rates of emergency room use.) However, when we used a Poisson model to reestimate the difference for total number of emergency room encounters during the year following home health admission, both the weighted and unweighted

[1]Medicare claims allowed us to identify both emergency room visits that lead to inpatient admissions (from inpatient claims that included a revenue center code for the emergency room) and outpatient emergency room visits (from outpatient claims with an emergency room revenue center code). Reimbursements for emergency room services were not distinguishable from other reimbursements on inpatient and outpatient claims.

[2]Table IV.1 and the other tables presented in this chapter contain estimates of the effect of per-episode payment for patients admitted to home health agencies during the first demonstration year, by four-month period and for the entire year. Companion tables in Appendix A show similar estimates during the eight months following admission for patients admitted to home health during demonstration year 2. Statistical comparisons of key impact estimates for patients admitted during demonstration years 1 and 2 (which do not appear in tables) suggested there were no significant differences in estimates between the two years.

TABLE IV.1

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN EMERGENCY ROOM USE DURING DEMONSTRATION YEAR 1

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Whether Any Emergency Room Encounter Resulting in Admission (Percentage)	21.3	-1.1* (0.09)	14.5	-1.8*** (0.01)	13.0	-0.8 (0.19)	**37.8**	**-2.7*** (0.01)**
Number of Emergency Room Encounters Resulting in Admission	0.28	-0.01 (0.12)	0.19	-0.02** (0.03)	0.17	-0.01 (0.22)	**0.63**	**-0.04** (0.03)**
Whether Any Emergency Room Encounter Not Resulting in Admission (Percentage)	19.1	-1.6** (0.02)	13.6	-0.5 (0.36)	12.7	-1.2* 0.07	**34.4**	**-1.9* (0.07)**
Number of Emergency Room Encounters Not Resulting in Admission	0.27	-0.03*** (0.01)	0.19	-0.01 (0.29)	0.18	-0.01 (0.20)	**0.64**	**-0.05* (0.06)**
Whether Any Emergency Room Encounters (Percentage)	34.4	-1.9** (0.02)	24.1	-1.8** (0.03)	22.0	-1.4* (0.08)	**55.0**	**-2.5** (0.04)**
Total Number of Emergency Room Encounters	0.55	-0.04*** (0.01)	0.38	-0.03** (0.05)	0.34	-0.02 (0.14)	**1.27**	**-0.09*** (0.01)**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any encounter" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) (See Appendix Table C.1 for estimated coefficients on all control variables for "number of emergency room encounters.") Observations were weighted so that each agency is represented equally.

There were 31,199 patients in the treatment group and 25,809 patients in the control group for the estimates reported in this table. The same sample was used for all four reference periods to avoid introducing potential bias to the impact estimates due to possible differences in mortality. Therefore, the sample in months 5 through 8 and months 9 through 12 includes patients who died during the previous periods. Approximately 10 percent of patients died by month 5, 15 percent by month 9, and 20 percent by month 12. Mortality rates (and survival time) were similar for treatment and control patients during each reference period.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

difference estimates suggested reductions in use that were of roughly similar size and statistically significant at the five percent level (consistent with the estimate presented in Table IV.1).

Patients of treatment and control agencies had roughly similar levels of inpatient service use during the year following home health admission. (See Tables IV.2 and A.2.) Just over half of all patients had an inpatient admission during the year after they entered home health. Hospitalization rates declined over time for both groups: while just under a third had an inpatient admission during the first four months following home health admission, only about a fifth had an admission during months 5 through 8 or months 9 through 12.[3] Patients spent an average of nine days in the hospital during the year. Medicare reimbursement for inpatient services during the year was about $8,000, or $667 per month. This level of reimbursement is substantially higher than the level for Medicare beneficiaries overall, which reflects the poorer health of home health patients. In 1996, inpatient reimbursement for a typical beneficiary was only about $197 per month (Health Care Financing Administration 1998).

As we observed for emergency room use, patients of treatment group agencies had lower levels of inpatient service use during each four-month period following home health admission than did control agency patients. These differences were consistent across outcomes and time periods and were also apparent for patients admitted to home health during the second demonstration year.

[3]Hospitalization rates (and most other types of service use) declined during the follow-up year. This was due in part to mortality. Roughly 10 percent of patients had died by the end of month 4, 15 percent by the end of month 8, and 20 percent by the end of the year following home health admission. Mortality rates and survival times during the follow-up year did not differ substantially for patients of treatment and control agencies. Deceased patients remained in the analysis sample with service use and reimbursement variables for them set to zero after their deaths to avoid introducing potential bias. Restricting the analysis sample to survivors would not have changed our conclusions about prospective payment's impact.

TABLE IV.2

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN INPATIENT HOSPITAL USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 1

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Whether Any Admission (Percentage)	31.9	−1.1 (0.20)	21.5	−1.5** (0.05)	19.2	−0.7 (0.31)	**52.1**	**−2.4** (0.04)**
Number of Admissions	0.49	−0.01 (0.51)	0.32	−0.02* (0.09)	0.28	−0.01 (0.58)	**1.09**	**−0.04 (0.21)**
Number of Days	3.9	−0.0 (0.75)	2.7	−0.1 (0.66)	2.3	−0.2 (0.30)	**8.9**	**−0.3 (0.41)**
Reimbursement (in Dollars)[b]	3,586	−55 (0.73)	2,312	−54 (0.69)	2,102	−141 (0.27)	**7,999**	**−250 (0.36)**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) (See Appendix Table C.1 for estimated coefficients on all control variables for "number of admissions.") Observations were weighted so that each agency is represented equally.

There were 31,199 patients in the treatment group and 25,809 patients in the control group for the estimates reported in this table. The same sample was used for all four reference periods to avoid introducing potential bias to the impact estimates due to possible differences in mortality. Therefore, the sample in months 5 through 8 and months 9 through 12 includes patients who died during the previous periods. Approximately 10 percent of patients died by month 5, 15 percent by month 9, and 20 percent by month 12. Mortality rates (and survival time) were similar for treatment and control patients during each reference period.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for inpatient services under Medicare Part B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

However, these differences were smaller than those observed for emergency room use, and only two were statistically significant. When inpatient models were reestimated without weighting each agency equally (not presented in tables), difference estimates were a bit smaller, and none were statistically significant. This reduction in estimate size and loss of statistical significance was due in part to the two smallest treatment group agencies having atypically low rates of inpatient service use.[4] Estimating differences with other statistical techniques--a Poisson model for number of admissions and a Tobit model for reimbursement (not presented)--also showed treatment-control differences that were statistically significant when observations were weighted to represent each agency equally but that were not statistically significant when observations were unweighted. These differences between weighted and unweighted results suggest that our inpatient findings should be viewed with particular caution.

Our estimates of the effect of per-episode payment on emergency room and inpatient service use clearly suggest that the reductions in home health visits observed under the demonstration did not lead to adverse patient outcomes, as would be reflected in the *increased* use of these services. In fact, we observed a modest, but robust, reduction in the use of emergency room services. This may have been a result of treatment agency nurses' efforts to improve patient self-care to allow patients to leave home health earlier. During site visits, treatment agencies reported that such efforts were under way. Agencies were clearly successful in reducing the length of home health episodes. Average episode length for control agency patients for whom 16 months of data were available was 131 days, compared with 98 days for similar patients of treatment agencies (Trenholm 1998). If self-care teaching helped patients monitor symptoms and medications more effectively, some emergency

[4]Only 21 and 33 percent of patients of the two smallest treatment group agencies were hospitalized during the year, compared to roughly half of all patients in the analysis sample.

room visits are likely to have been avoided.[5] It seems unlikely that improved patient teaching that was part of a demonstration more directly concerned with reducing home health visits could have led to large reductions in emergency room visits; however, the modest reductions we observe here seem credible. Reductions in emergency room encounters included reductions in emergency room encounters that led to inpatient admissions. Thus, while the observed treatment-control difference in inpatient use should be viewed with caution, it has a plausible explanation.

B. SKILLED NURSING FACILITY AND HOSPICE SERVICES

Even if per-episode payment did not adversely affect care quality, it could have caused care to be shifted from home health to some other setting (such as a nursing home or hospice), particularly if a home health agency viewed a patient as particularly costly to serve. However, our analysis suggests that this did not happen.

We found no evidence that care in skilled nursing facilities was substituting for the observed reduction in home health visits. (See Tables IV.3 and A.3.) Roughly 20 percent of treatment and control agency patients had a skilled nursing facility stay during the year following home health admission. Patients in both groups spent about six and a half days, on average, in a skilled nursing facility. Average reimbursement for the period was just over $1,900, or about $160 per month. In 1996, skilled nursing facility reimbursement was about $24 per month for the typical Medicare beneficiary, which once again reflects the poorer health of home health patients (Health Care Financing Administration 1998).

[5]The evaluation also included a telephone survey of a sample of demonstration home health patients eight months after admission. The survey included a question about the perceived quality of patient education provided by home health staff. There was no treatment-control difference in responses to this question (Chen 1999). However, the question may have been worded too broadly to pick up specific improvements in symptom monitoring or treatment adherence.

TABLE IV.3

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN SKILLED NURSING FACILITY AND HOSPICE USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 1

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Skilled Nursing Facility								
Whether Any Admission (Percentage)	10.0	−0.1 (0.82)	8.0	−0.0 (0.95)	7.2	−0.2 (0.62)	**20.0**	**−0.1 (0.88)**
Number of Admissions	0.12	0.00 (0.98)	0.09	−0.00 (0.90)	0.08	0.00 (0.41)	**0.29**	**0.0 (0.80)**
Number of Days	2.4	−0.1 (0.61)	2.2	0.0 (0.86)	2.1	−0.2 (0.18)	**6.6**	**−0.2 (0.51)**
Reimbursement (in Dollars)[b]	717	−29 (0.63)	599	−4 (0.94)	602	−76** (0.05)	**1,918**	**−110 (0.38)**
Hospice								
Whether Any Admission (Percentage)	3.0	−0.5 (0.15)	1.7	0.0 (0.98)	1.5	−0.4** (0.05)	**5.6**	**−0.8 (0.11)**
Number of Days	0.7	0.0 (0.75)	0.8	−0.1 (0.50)	0.7	−0.0 (0.81)	**2.1**	**−0.1 (0.83)**
Reimbursement (in Dollars)[b]	79	0 (0.98)	85	−10 (0.35)	79	−11 (0.37)	**244**	**−20 (0.46)**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE IV.3 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) (See Appendix Table C.2 for estimated coefficients on all control variables for "any skilled nursing facility admission.") Observations were weighted so that each agency is represented equally.

There were 31,199 patients in the treatment group and 25,809 patients in the control group for the estimates reported in this table. The same sample was used for all four reference periods to avoid introducing potential bias to the impact estimates due to possible differences in mortality. Therefore, the sample in months 5 through 8 and months 9 through 12 includes patients who died during the previous periods. Approximately 10 percent of patients died by month 5, 15 percent by month 9, and 20 percent by month 12. Mortality rates (and survival time) were similar for treatment and control patients during each reference period.

[a]The *p*-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for skilled nursing facility and hospice services under Medicare Part B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

We also found no evidence that reductions in home health visits shifted care to hospices. Fewer than six percent of treatment and control agency patients used hospice services during the year following home health admission, and reimbursement averaged just under $250 for the year.

C. NONDEMONSTRATION HOME HEALTH AND ALL PART B HOME HEALTH SERVICES

A number of events could lead a patient of a treatment group home health agency to receive services from another agency during the year following home health admission. Some of these events are not related to the demonstration and thus should be equally likely to occur to patients of control agencies. For example, hospital-based home health agencies sometimes provide services to patients who require home health care following hospitalization, even if the patient was in the care of another agency before the hospitalization. People who were vacationing in one area of the country when they become ill and were admitted to a demonstration agency might return home and begin receiving care from another agency. (This seems particularly likely for demonstration agencies in the sunbelts of California, Florida, or Texas.) In other cases, agencies that do not provide the full spectrum of home health services might share a patient with another agency that provides needed services, such as medical social services or occupational therapy (although demonstration agency staff reported that this seldom was true for them).

Earlier discharge of patients by treatment agencies (an incentive under per-episode payment) may also lead to the increased use of home health services provided by other agencies. However, earlier discharge (and subsequent use of other home health services) could reflect either better or poorer care from treatment agencies or could be independent of care quality. Earlier discharge would reflect better care if it resulted from more efficient patient teaching and better coordination with community services. Alternatively, the care provided may be of comparable quality but limited to what is truly necessary. Nonetheless, patients may become accustomed to receiving home health

care and, in a highly competitive home health market, may be able to find other home health agencies to provide services--even those no longer strictly necessary. On the other hand, if treatment agencies discharged patients inappropriately early, the increased use of other home health care could reflect poorer care, particularly if it were accompanied by increases in the use of other services. Finally, earlier discharge, whether or not it reflected a change in care quality, would provide a greater window of opportunity for a nondemonstration admission during the patient follow-up year. All these events--those induced by per-episode payment, as well as those independent of the demonstration--are reflected in our measures of home health provided to demonstration patients by nondemonstration agencies.[6]

We found that treatment agencies discharged patients an average of 33 days sooner than did control agencies over the 16 months following home health admission (Trenholm 1998). If this large difference in episode length were the result of inappropriately early discharges, it probably would have led to a large increase in admissions to nondemonstration home health agencies, increases in the visits such agencies provide, and increases in the use of other services (such as those provided in emergency rooms, inpatient hospitals, or skilled nursing facilities).

During the year following a demonstration home health agency admission, however, patients of treatment group agencies had only somewhat higher levels of use of home health services from nondemonstration agencies than did control agency patients. (See Tables IV.4 and A.4.) During the year, 20 percent of the treatment agency patients had received services from a nondemonstration

[6]In this report a "nondemonstration" agency refers to any agency other than the treatment or control agency that originally provided the identifying home health admission. In almost all cases, this was an agency not participating in the demonstration. A companion report on the selection and retention of patients by demonstration home health agencies will investigate specific reasons for the use of nondemonstration home health services.

TABLE IV.4

ESTIMATED DIFFERENCES BETWEEN PER EPISODE PAYMENT AND COST REIMBURSEMENT IN NONDEMONSTRATION HOME HEALTH USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 1

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Whether Any Admission (Percentage)	7.7	1.1 (0.13)	9.7	1.8* (0.08)	10.9	1.8** (0.03)	**16.9**	**2.8** (0.02)**
Number of Visits	3.21	0.62 (0.21)	5.19	0.86 (0.26)	6.29	0.98 (0.20)	**14.68**	**2.46 (0.20)**
Reimbursement (in Dollars)[b]	235	42 (0.19)	366	66 (0.22)	440	82 (0.16)	**1,042**	**191 (0.16)**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) (See Appendix Table C.2 for estimated coefficients on all control variables for "any admission.") Observations were weighted so that each agency is represented equally.

There were 31,199 patients in the treatment group and 25,809 patients in the control group for the estimates reported in this table. The same sample was used for all four reference periods to avoid introducing potential bias to the impact estimates due to possible differences in mortality. Therefore, the sample in months 5 through 8 and months 9 through 12 includes patients who died during the previous periods. Approximately 10 percent of patients died by month 5, 15 percent by month 9, and 20 percent by month 12. Mortality rates (and survival time) were similar for treatment and control patients during each reference period.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for nondemonstration home health services under Medicare Part B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

agency, compared with 17 percent of control agency patients. This difference was statistically significant. However, treatment-control differences in the number of visits received and reimbursement for those visits were not statistically significant for any of the analysis periods. On average, patients received about 16 visits from nondemonstration home health agencies during the year following a demonstration agency admission, and Medicare reimbursement for those visits was about $1,100. (Estimated differences for patients admitted during the second year of the demonstration also suggested slightly higher use levels, but the year 2 differences were smaller and none were statistically significant.)

These results suggest that, while the likelihood of receiving nondemonstration home health services was somewhat greater for treatment agency patients, treatment agency patients receiving such visits did not receive more visits than their control group counterparts. The following chart, which displays the average number of visits provided by demonstration and nondemonstration agencies *to the subsample of patients who had visits from both types of agencies,* further supports this finding. Treatment and control agency patients who used nondemonstration home health

ESTIMATED DIFFERENCES FOR PATIENTS WHO RECEIVED VISITS FROM DEMONSTRATION AND NONDEMONSTRATION AGENCIES DURING DEMONSTRATION YEAR 1

	Control Group Mean	Difference (*p*-Value)	Difference as Percent of Control Mean
Number of Visits During Months 1 Through 12			
Demonstration agency	49.16	-17.26*** (0.00)	-35.1
Nondemonstration agency	78.72	-1.26 (0.79)	-1.6
All agencies	127.88	-18.24*** (0.00)	-14.3
Number of Patients			
Treatment		6,213	
Control		4,186	

***Significantly different from zero at the .01 level, two-tailed test.

agencies during the year following a demonstration agency admission received nearly identical numbers of visits from the nondemonstration agency during that year--on average, just under 80.

Although nondemonstration visits did not differ for treatment and control agency patients, treatment agencies appeared to have reduced their (demonstration) visits to patients who received nondemonstration home health care more than to patients who did not receive such care. Trenholm (1998) found that treatment agencies reduced the number of visits provided overall by about 25 percent. However, the chart above shows that visits were reduced by 35 percent for patients receiving nondemonstration home health. The larger percentage reduction for these patients resulted from the lower mean number of demonstration visits provided to control patients receiving nondemonstration home health: 49, compared with 76 for control agency patients overall.

A likely explanation for the lower control group mean could be that patients receiving services from more than one home health agency on average were much sicker:

- This seems to be borne out by the greater number of total home health visits such patients received during the follow-up year. Among those patients initially admitted to agencies in the demonstration's control group who went on to receive services from a nondemonstration agency, the total number of home health visits received in a year was 128 (49 demonstration and 79 nondemonstration agency visits), on average, compared to 91 (76 demonstration and 15 nondemonstration agency visits), on average, for all control agency patients.

- Because patients receiving services from more than one agency were sicker, they were more likely to be hospitalized. More than 70 percent of the subsample of patients receiving demonstration and nondemonstration agency visits had a hospitalization during the year following demonstration agency admission, compared with roughly 50 percent for all patients.

- Because they were more likely to be hospitalized, they were more likely to be picked up by (nondemonstration) hospital-based home health agencies. As a result, demonstration agency visits were fewer.

Because we saw no evidence of adverse patient consequences as reflected in increased use of emergency room, inpatient hospital, or skilled nursing facility services or visits from nondemonstration agencies, we conclude that the observed treatment-control difference in the percentage of patients receiving visits from nondemonstration agencies was *not* a result of inappropriate reductions in visits by treatment agencies. Nevertheless, treatment-control differences in nondemonstration home health use and reimbursement were consistently positive. This may be explained in part by the fact that earlier discharge from demonstration home health agencies gave treatment agency patients a greater window of opportunity for a nondemonstration home health admission during the follow-up year. (That is, patients who remain in home health care cannot be admitted to other home health agencies--except in rare instances of patient sharing--whereas patients discharged earlier are at risk of such admissions.) In addition, the differences presented in Table IV.4 seem to be sensitive to a few treatment group agencies of various sizes with unusually high proportions of patients receiving care from other agencies, including one that was very large.[7] (These high rates of nondemonstration home health use appear to be unrelated to demonstration home health episode length or visits received. Because these few agencies were of different sizes, models with alternative weighting schemes produced similar impact estimates.) Therefore, although we observe somewhat higher rates of nondemonstration home health use among treatment agency patients, we believe this reflects the longer period during which they could have incurred a

[7]The mean rate of nondemonstration agency use for patients of this large treatment agency was 56 percent, compared with 20 percent overall. Staff at this agency reported that the home health environment in their city was extremely competitive. They noted that hospitals routinely recruited the agency's patients for hospital-based agencies when the patients were hospitalized and that other agencies inappropriately admitted patients they had discharged. Moreover, in mid-1997, HCFA erroneously published a notice in city newspapers that the agency was going to be shut down for violations of Medicare's Conditions of Participation. This led to a loss of patients, who very likely turned to other home health agencies for care (at least for a time).

nondemonstration home health admission, as well as atypically high use rates among a small number of treatment group agencies, rather than inappropriately early discharge.[8]

Agencies receiving per-episode payment also had a financial incentive to shift services from Part A to Part B coverage, to the extent possible, to increase per-visit payments. Shifting of services was merely a secondary concern because, under regulations in effect during the demonstration, per-visit payment for Part B home health applied to a relatively small number of patients.[9] In fact, only about a tenth of a percent of all patients received services from demonstration agencies that were paid per-visit under Part B during the risk period or during the rest of the year following admission, with no substantial difference in use between patients of treatment and control agencies. (Impact estimates for Part B home health per se do not appear in tables.)

D. PART B SERVICES

Per-episode payment had no effect on the use of other Part B services. (See Tables IV.5 and A.5.) Nearly 80 percent of patients from treatment and control agencies used outpatient hospital facilities, and average outpatient reimbursement was about $1,300 during the year following home

[8]It might seem that the higher likelihood of nondemonstration home health admission for treatment agency patients could have led to their observed modest reductions in emergency room and inpatient service use noted in Section A of this chapter. This, however, was not the case. When impacts on the total number of emergency room encounters and the likelihood of an inpatient admission were reestimated on the subsample of patients who had a nondemonstration home health admission, we found no statistically significant treatment-control differences.

[9]Treatment group agencies were paid on a per-episode basis for patients who only had Part B coverage. They could only receive per-visit payments under Part B for the very few patients who (1) had been receiving therapy visits under a home health plan of treatment but were no longer homebound and wanted to continue with the same therapists in an outpatient setting, or (2) were in skilled nursing facilities but had exceeded their lifetime skilled nursing facility coverage limits and received therapy services under Part B home health.

TABLE IV.5

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN PART B USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 1

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Outpatient Hospital[b]								
Whether Any Services (Percentage)	63.8	−1.2 (0.51)	51.1	−0.3 (0.83)	46.9	−0.1 (0.92)	**79.4**	**−0.8 (0.55)**
Reimbursement (in Dollars)	516	−4 (0.87)	414	−9 (0.68)	369	13 (0.56)	**1,299**	**−1 (0.99)**
Physician and Other Practitioner								
Whether Any Visit (Percentage)	91.6	−0.4 (0.53)	79.2	−0.3 (0.76)	73.9	0.2 (0.85)	**95.7**	**−0.1 (0.84)**
Reimbursement (in Dollars)	911	−14 (0.75)	618	9 (0.77)	555	−3 (0.92)	**2,084**	**−8 (0.93)**
Durable Medical Equipment								
Whether Any Purchase (Percentage)	46.5	−0.5 (0.71)	32.9	0.1 (0.91)	30.2	0.2 (0.78)	**56.3**	**−0.4 (0.71)**
Reimbursement (in Dollars)	300	−9 (0.61)	222	−5 (0.75)	210	−12 (0.44)	**732**	**−26 (0.54)**

TABLE IV.5 *(continued)*

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Other Part B Services[c]								
Whether Any Use (Percentage)	82.7	−1.3 (0.26)	67.1	−1.7 (0.17)	62.3	−1.3 (0.32)	**93.5**	**−0.9 (0.21)**
Reimbursement (in Dollars)	313	8 (0.64)	229	−8 (0.59)	206	−8 (0.50)	**749**	**−8 (0.80)**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any services," "any visit," "any purchase," and "any use" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

There were 31,199 patients in the treatment group and 25,809 patients in the control group for the estimates reported in this table. The same sample was used for all four reference periods to avoid introducing potential bias to the impact estimates due to possible differences in mortality. Therefore, the sample in months 5 through 8 and months 9 through 12 includes patients who died during the previous periods. Approximately 10 percent of patients died by month 5, 15 percent by month 9, and 20 percent by month 12. Mortality rates (and survival time) were similar for treatment and control patients during each reference period.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Includes both emergency and nonemergency visits to outpatient hospitals, as well as use of laboratory and radiology services.

[c]Includes diagnostic laboratory and radiology services (including pathologist and radiologist services) from nonhospital providers, supplies and devices, mammography, ambulance, covered medications, blood, and vaccines.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

health admission. (In 1996, 62 percent of all Medicare beneficiaries had an outpatient hospital claim; these visits were primarily for diagnostic laboratory and radiology services, medical/surgical supplies, [ambulatory] operating room services, and services related to end-stage renal disease [Health Care Financing Administration 1998].)

Almost all patients (96 percent) visited their physicians (or other practitioners) during the year following home health admission, and average reimbursement for physician services was just over $2,000. Physicians often order diagnostic tests when patients visit; therefore, during the year, more than 90 percent of patients used "other" Part B services, which are dominated by diagnostic laboratory and radiology services furnished by nonhospital providers. Other Part B services also include supplies and devices, mammography, ambulance, covered medications and vaccines, and blood. Average reimbursement for these services was about $750. Finally, more than half of all patients (56 percent) received durable medical equipment (with an average reimbursement of just over $700) during the year following home health admission.

E. TOTAL MEDICARE REIMBURSEMENT

Because per-episode payment had modest, but offsetting, effects on the use of specific Medicare-covered services (small reductions in emergency room and inpatient service use but a modest increase in nondemonstration home health use), it had no effect on Part A, Part B, or overall Medicare reimbursement. (See Tables IV.6 and A.6.) On average, patients of treatment and control agencies had Part A reimbursement (exclusive of payment to demonstration home health agencies) of about $11,200 and Part B reimbursement of about $4,800 during the year following home health admission, for a total of about $16,000 (or about $1,300 per month).

We have not included demonstration home health reimbursement in our totals because regression-adjusted estimates of the effect of per-episode payment on home health reimbursement

TABLE IV.6

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN TOTAL MEDICARE REIMBURSEMENT DURING DEMONSTRATION YEAR 1
(in Dollars)

	Months 1 Through 4		Months 5 Through 8		Months 9 Through 12		**Months 1 Through 12**	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	**Control Group Mean**	**Difference (p-Value)[a]**
Total Medicare Part A Reimbursement[b]	4,671	-22 (0.91)	3,381	-2 (0.99)	3,239	-143 (0.32)	**11,292**	**-168 (0.62)**
Total Medicare Part B Reimbursement	2,041	-17 (0.77)	1,483	-11 (0.84)	1,340	-9 (0.88)	**4,864**	**-37 (0.79)**
Total Medicare Reimbursement[c]	6,712	-17 (0.94)	4,864	4 (0.98)	4,579	-138 (0.44)	**16,156**	**-151 (0.72)**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented in this table were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) (See Appendix Table C.3 for estimated coefficients on all control variables for "Part A reimbursement" and "Part B reimbursement.") Observations were weighted so that each agency is represented equally.

There were 31,199 patients in the treatment group and 25,809 patients in the control group for the estimates reported in this table. The same sample was used for all four reference periods to avoid introducing potential bias to the impact estimates due to possible differences in mortality. Therefore, the sample in months 5 through 8 and months 9 through 12 includes patients who died during the previous periods. Approximately 10 percent of patients died by month 5, 15 percent by month 9, and 20 percent by month 12. Mortality rates (and survival time) were similar for treatment and control patients during each reference period.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

[c]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

are not informative.[10] Reimbursement to treatment group agencies for the first 120 days following admission was set, according to the demonstration, at predetermined levels based on agency reimbursement patterns during the year before the agency joined the demonstration and, thus, was not a function of current patient characteristics. By contrast, control group payment during the first 120 days was based on the number of visits provided to current patients.[11] Thus, because the payment mechanisms for patients of treatment and control agencies were not comparable during the at-risk period, estimating regression-adjusted impacts on home health reimbursement during the entire follow-up year was not appropriate.

We did, however, develop a "hybrid" estimate of the impact of per-episode payment on demonstration home health reimbursement. This estimate was the sum of a nonregression-adjusted treatment-control difference for the four-month, at-risk period (covered by per-episode payment) and a regression-adjusted difference for the rest of the follow-up year (months 5 through 12, when treatment and control agencies were both paid on a per-visit basis). While the treatment-control difference in demonstration home health reimbursement during the at-risk period was statistically negligible, the difference for the rest of the year was large and statistically significant. The reason for the difference during the rest of the year is that, during that time, a much smaller number of treatment agency patients were still receiving visits from demonstration agencies. For example, 25 percent of control agency patients were receiving visits from demonstration home health agencies at the end of month 8, compared with 18 percent of treatment agency patients. The comparable

[10]As noted, Trenholm (1998) discussed the effect of per-episode payment on the use of demonstration home health services. Estimates of the effect of per-episode payment on the cost of demonstration home health care (that is, the cost to the home health agency providing care) will be the subject of a companion report.

[11]Reimbursement to treatment and control agencies *following* day 120 was based on the number of visits provided to current patients, and, thus, was a function of their characteristics.

numbers for year end were 15 and 10 percent for control and treatment agency patients, respectively. As a result, reimbursement for home health services provided by demonstration agencies during the entire year following admission averaged $5,050 for patients of control group agencies, compared with $4,439 for patients of treatment group agencies.

We noted earlier that a somewhat higher proportion of treatment agency patients used nondemonstration home health services during the year following demonstration agency admission. However, the cost of these nondemonstration home health services for the treatment group was not high enough to offset the increased cost of demonstration home health care for the control group. Total demonstration and nondemonstration home health reimbursement was just under $6,100 for patients of control agencies and just under $5,700 for treatment agency patients.

F. SUMMARY

We found no evidence that per-episode payment for home health services increased the use of other types of Medicare-covered services overall. A pattern of a somewhat increased likelihood of nondemonstration home health admission seemed to have had two causes. First, although earlier discharges among treatment agency patients appeared to have been appropriate, the earlier discharge provided a larger window of opportunity during which a treatment agency patient could be admitted to a nondemonstration home health agency. Second, a few treatment agencies had much higher rates of nondemonstration use than did any control agencies. This increase in nondemonstration home health use did not increase the overall (demonstration and nondemonstration) number of home health visits received or Medicare home health spending. However, we did observe a pattern of *reduced* emergency room (and possibly inpatient service) use, which we hypothesize resulted from improved self-care teaching by treatment agency nurses who were being urged by agency management to facilitate earlier discharge.

V. SUBGROUP IMPACTS ON THE USE OF AND REIMBURSEMENT FOR MEDICARE-COVERED SERVICES

In this chapter, we examine whether particular agency or patient characteristics affected the use of and reimbursement for Medicare-covered services. Different types of home health agencies may have responded differently to demonstration incentives, or agencies may have responded differently for certain types of patients. These differences in agency behavior may, in turn, have led to differences in the use of other Medicare-covered services. For example, we found that agencies that historically provided a large number of visits, given their patient mix, made the deepest cuts in visits under the demonstration. Cuts were also deeper for small agencies, freestanding agencies, and agencies with costs above the Medicare limits, as well as for patients with high expected costs (Trenholm 1998). We are particularly interested in whether patients of such agencies were more likely to suffer adverse health outcomes or to shift their care to other settings, as reflected in their increased use of other Medicare-covered services. In addition, we are interested in whether the general lack of treatment-control differences in Medicare service use reported in Chapter IV may be obscuring offsetting differences for particular groups of patients. We would also like to know whether particular groups drive the overall reduction in the use of emergency room services or the overall increase in nondemonstration home health care. To investigate these possibilities, we reestimated key demonstration impacts for a series of agency- and patient-level subgroups we have hypothesized to be associated with differences in agency behavior.

A. HYPOTHESES CONCERNING SUBGROUP IMPACTS

Our basic hypothesis concerning the impact of home health prospective payment on the use of and reimbursement for Medicare-covered services is that a reduction in demonstration home health visits will lead to an increase in the use of other services. Our hypothesis concerning subgroup impacts similarly assumes that patients who belong to subgroups that experience the deepest reductions in home health visits will have the largest increases in their use of other services.

We used five agency-level characteristics to define subgroups: (1) practice pattern, (2) profit status, (3) size, (4) auspice, and (5) base-year costs. From these characteristics, we defined the following subgroup pairs:

- ***High-Use Versus Low-Use Practice Pattern.*** These subgroups are defined from a case-mix-adjusted index of agencies' service provision in the base quarter (as described in Chapter II). High-use agencies have an index value above the median value; low-use agencies are at or below the median value.
- ***For-Profit Versus Nonprofit.*** These terms are defined using the agency's base-year cost report.
- ***Small Versus Large.*** Small agencies provided fewer than 30,000 visits in the base year; large agencies provided more than 30,000 visits (according to base-year cost reports).
- ***Hospital-Based Versus Freestanding.*** These terms are defined using the agency's base-year cost report.
- ***Below Versus Above the Cost Limits.*** These subgroups are defined from a comparison of each agency's base-year cost per visit to the Section 223 Limits.

In general, we hypothesized that the agencies that would make the deepest cuts to visits would be those that provided more visits prior to the demonstration (for example, agencies with high-use practice patterns), those with stronger incentives to generate a profit or surplus (for-profit agencies and agencies over the cost limits), and those that might be able to respond more quickly to the

demonstration's incentives (small agencies and hospital-based agencies). In turn, we expected patients of agencies that cut visits more deeply to use other Medicare services more.

We also examined subgroups defined by three patient characteristics: (1) ability to take oral medications independently, (2) access to caregivers, and (3) expected costs at home health admission.[1] The subgroup pairs that we defined from these characteristics were:

- ***Independent Versus Not Independent in Taking Oral Medications.*** This was defined based on the demonstration agency nurse assessment at admission to home health.
- ***Caregiver Available Versus No Caregiver Available.*** Patients are categorized as having a caregiver available if they have a live-in informal caregiver, if they have paid help, or if they live in an assisted-living facility. Otherwise they are categorized as having no caregiver.
- ***High Versus Low Expected Costs.*** A patient is defined as having high expected costs if he or she falls in the top quartile of predicted costs for all patients in our sample.[2]

We hypothesized that agencies would be likely to cut visits more deeply for patients who were better able to manage their own care (as reflected by their ability to manage oral medications), had consistently available caregivers, or seemed likely to require a lot of home health visits. Again, patients who experienced the largest cuts in home health visits were expected to use other Medicare

[1]All the information used to form patient subgroups came from the quality assurance (QA) forms that the agencies completed at admission. Because these data are available for only a subsample of patients, the sample for this analysis includes about a third of the number of admissions used in the main analysis (and the evaluation of agency-level subgroups). In addition, we dropped all the observations for 10 agencies because their QA-based subsample included a very small number of admissions (fewer than 20). We expect that these changes to the sample had little effect on the accuracy of our results. As we note in the sensitivity analysis discussed in Chapter III, the main impacts differed only slightly when using this QA-based subsample. However, the precision of our estimates is weaker than it would have been had we used the full sample.

[2]The variables used to calculate predicted costs are those identified from Phillips et al. (1992) as increasing costs for the average patient by more than 30 percent.

services more. (See Trenholm 1998 for a more detailed discussion of hypothesized impacts of the demonstration on home health use for these subgroups.)

B. SUBGROUP IMPACT ESTIMATES

We investigate the possibility that the impact of prospective payment differed for certain types of agencies or patients in order to answer three questions:

1. Are the types of patients for whom the reduction in home health visits was deepest more likely to suffer adverse outcomes or shift care to other settings?
2. Are offsetting impacts for particular types of agencies or patients masked by the lack of impacts we observe overall for most measures of Medicare service use and reimbursement?
3. Is the observed reduction in emergency room services or the observed increase in nondemonstration home health use driven by impacts for particular types of agencies or patients?

Trenholm (1998) found that, overall, patients of agencies receiving prospective payment received 18 fewer home health visits than patients of control group agencies during the year following admission. However, as the chart below reflects, Trenholm also concluded that the reduction was greater for certain patients: those from high-use, small, or freestanding agencies; those from agencies that were over the

Demonstration Year 1	Overall	High-Use Agencies	Small Agencies	Freestanding Agencies[a]	Agencies Over Cost Limits	Patients with High Expected Costs
Mean number of demonstration home health visits for control group	76	91	102	80	69	109
Estimated reductions in number of visits	-18	-24	-31	-19	-31	-25
Estimated reduction as percent of control group mean	-24	-26	-30	-24	-45	-23

[a] The estimated reductions in the number (and percent) of home health visits for freestanding agencies--at 19 visits and 24 percent--are close to the estimated reduction for all agencies because freestanding agencies dominate the overall sample: 77 out of 87 agencies in the analysis sample were freestanding. However, the reduction for freestanding agencies is nearly twice as large as that for hospital-based agencies, which reduced demonstration agency visits by only 10, on average. In addition, freestanding agencies made much larger reductions than hospital-based agencies in the provision of aide visits.

cost limits; and those who were expected to incur high home health costs. Although for-profit agencies also cut more visits than did nonprofits, this reduction was confounded by the fact that for-profit agencies often had high-use practice patterns. Thus, in models that control for practice pattern (such as those used in the analysis of Medicare service use and reimbursement), there was no significant difference in impacts between patients of for-profit and nonprofit agencies. Trenholm found more limited evidence that the reductions noted for small and freestanding agencies were also confounded by differences in practice patterns.[3]

For each subgroup, we estimated program impacts on six key Medicare service outcomes: (1) total number of emergency room encounters, (2) number of inpatient admissions, (3) whether admitted to a skilled nursing facility, (4) whether admitted to nondemonstration home health, (5) Medicare Part A reimbursement, and (6) Medicare Part B reimbursement. We then conducted statistical tests to determine (1) whether the program impact for a given subgroup was significantly different from zero; and (2) whether a significant difference existed in the impacts between subgroup pairs (for example, between estimated impacts for patients of for-profit and nonprofit agencies).

The tables presented in this chapter summarize our analysis of the impact of prospective payment on key outcomes by subgroup. Pluses and minuses reflect the sign of each subgroup impact; asterisks reflect the level of statistical significance of the subgroup impacts. Shading indicates that the difference between subgroup impacts was statistically significant at the five percent level. The rightmost column of each table summarizes the conclusions of Trenholm's subgroup analysis for reductions in demonstration home health visits. (Double check marks indicate that the

[3]Trenholm's conclusions concerning the effect of per-episode payment on home health visits for particular subgroups were based not only on reductions in total visits, but also on reductions in visits by discipline and in episode length. For example, the conclusion that freestanding agencies reduced visits more than hospital-based agencies was partially based on freestanding agencies having made a much larger reduction than hospital-based agencies in the provision of aide visits.

differences within subgroups and across subgroup pairs were statistically significant at the .05 level. Single check marks indicate convincing, though weaker, evidence of subgroup differences.) Tables B.1 through B.8 in Appendix B contain the numerical impact estimates upon which the summary tables are based.

1. Emergency Room Encounters and Inpatient Admissions

Overall, during the year following home health admission, patients of treatment group agencies used emergency room services somewhat less than their control agency counterparts, culminating in a reduction in total emergency room encounters of about seven percent. We attributed this reduction to treatment agency efforts to improve the teaching of patient self-care to allow patients to leave home health earlier. Subgroup analysis did not provide any conclusive evidence that this reduction was driven by reductions for particular types of agencies or patients. Individual subgroup impact estimates were all negative and many were statistically significant, but we observe no statistically significant differences within subgroup pairs. (See Table V.I.)

Overall, we found that prospective payment had no statistically significant effect on the use of and reimbursement for inpatient services during the year following home health admission. However, we did observe a pattern of small, negative treatment-control differences that may have resulted from the reduction in emergency room encounters. Estimates of the effect of prospective payment on the number of inpatient admissions by subgroup suggest that this pattern may have been driven by a greater than average reduction in admissions for patients who had no live-in informal or paid caregivers. It is not clear why we observe this reduction. We hypothesized that any subgroup difference in the use of Medicare services that could be attributed to prospective payment would be driven by a greater than average reduction in the provision of home health visits. However,

TABLE V.1

SUMMARY OF ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN NUMBERS OF EMERGENCY ROOM ENCOUNTERS AND INPATIENT ADMISSIONS DURING MONTHS 1 THROUGH 12, BY SUBGROUP

	Number of Emergency Room Encounters	Number of Inpatient Admissions	Number of Demonstration Home Health Visits
Overall Control Mean/Estimated Difference	1.27/-0.09***	1.09/-0.04	76/-18***
Agency Subgroups			
Practice Pattern			
High-use	-**	-	✓✓
Low-use	-	-	
Profit Status			
For-profit	-*	-**	
Nonprofit	-**	+	
Size			
Small	-	-	✓
Large	-***	-	
Auspice			
Hospital-based	-	-	
Freestanding	-**	-	✓
Cost			
Under limit	-**	-	
Over limit	-**	-	✓
Patient Subgroups			
Management of Oral Medications			
Can take independently	-***	-	
Cannot take independently	-	-	
Live-In or Paid Caregiver			
Has no caregiver	-**	-***	
Has caregiver	-*	+	
Expected Home Health Cost			
High	-	-	✓
Low	-***	-*	

TABLE V.1 *(continued)*

SOURCE: Appendix Tables B.1 through B.8.

NOTE: ✓✓ means differences in demonstration home health use within subgroups and across subgroups were statistically significant at the .05 level.
✓ means convincing, though weaker, evidence of subgroup differences.

*Subgroup impact significantly different from zero at the .10 level, two-tailed test.
**Subgroup impact significantly different from zero at the .05 level, two-tailed test.
***Subgroup impact significantly different from zero at the .01 level, two-tailed test.

 means the difference between subgroup impacts is significantly different from zero at the .05 level.

Trenholm (1999) observed no statistically significant difference in home health visit reductions for subgroups defined by caregiver availability.

On the one hand, among the 16 statistical comparisons of subgroup impact estimates in Table V.1, it is not unreasonable to find one statistically significant simply by chance. (This is sometimes referred to as Type 1 error.) On the other hand, treatment group use of emergency room, inpatient, and skilled nursing facility services is lower than control group use of these services by a larger margin for patients without consistently available caregivers than for patients with caregivers, though few estimates are statistically significant. In Chapter IV, we argued that the reductions in emergency room use for the full sample may have been due to treatment agencies' efforts to improve patient self-care with the goal of reducing their home health visits. It is possible that patients without caregivers were healthier or had fewer physical or cognitive limitations and, thus, were able to respond more effectively to self-care teaching, even if this teaching did not achieve the immediate goal of reducing home health visits. If such patients were, in fact, taking better care of themselves following home health (for example, doing a better job of adhering to treatment recommendations and monitoring symptoms), this may have led to the pattern that we observe of larger, negative, treatment-control differences in service use.

A similar argument may be made for the subgroup of patients who were able to manage their oral medications independently and for patients of hospital-based agencies. Patients who can manage oral medications are also likely to be healthier or have fewer physical or cognitive limitations than those who cannot and, thus, may be better able to benefit from home health agency self-care teaching. Similarly, it seems plausible that hospital-based agencies would tend to have a mix of patients that was more likely to have short-term postacute needs and, thus, have fewer limitations that would lessen the effectiveness of teaching efforts. In fact, we see patterns of

negative estimates consistent with greater reductions in emergency room, inpatient, and skilled nursing facility services for these groups, though, again, few were statistically significant, and prospectively paid agencies generally failed to cut visits more for these subgroups.

2. Skilled Nursing Facility and Nondemonstration Home Health Admissions

Overall, we found that prospective payment had no statistically significant effect on the use of and reimbursement for skilled nursing facility services during the year following home health admission. The overall lack of impact on the likelihood of nursing home admission may have masked differences for patients of agencies characterized by practice pattern and auspice and for patients with differing ability to manage oral medications. (See Table V.2.) Among agencies that had low-use practice patterns and hospital-based agencies, patients of treatment group agencies were less likely than their control counterparts to be admitted to a nursing home. Among patients who could take oral medications independently, patients of treatment group agencies were less likely to be admitted to a nursing home. These findings run counter to our expectation that patients who experienced the greatest reductions in home health visits would be most likely to increase their use of skilled nursing facilities to substitute for that care. Under this assumption, we expected to see an increased likelihood of nursing home admission for patients of freestanding agencies and agencies with high-use practice patterns. In fact, among these agency subgroups, we see no statistically significant differences between treatment and control agency patients.[4] As noted in the previous section, the larger, negative, treatment-control differences in the likelihood of admission for patients of hospital-based agencies and those who can manage oral medications may be part of a broader

[4]The treatment-control difference in the likelihood of an admission for patients of high-use agencies was positive and just under two percentage points, but it was not significant at the 10 percent level. The treatment-control difference for patients of freestanding agencies was essentially zero.

TABLE V.2

SUMMARY OF ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN THE LIKELIHOOD OF SKILLED NURSING FACILITY AND NONDEMONSTRATION HOME HEALTH ADMISSIONS DURING MONTHS 1 THROUGH 12, BY SUBGROUP

	Any Skilled Nursing Facility Admission (Percent)	Any Nondemonstration Home Health Admission (Percent)	Number of Demonstration Home Health Visits
Overall Control Mean/Estimated Difference	20.0/-0.1	16.9/2.8**	76/-18***
Agency Subgroups			
Practice Pattern			
High-use	+	+	✓✓
Low-use	-**	+***	
Profit Status			
For-profit	-	+	
Nonprofit	+	+**	
Size			
Small	-	+***	✓
Large	+	+	
Auspice			
Hospital-based	-**	+	
Freestanding	+	+***	✓
Cost			
Under limit	-	+***	
Over limit	+	-	✓
Patient Subgroups			
Management of Oral Medications			
Can take independently	-*	+	
Cannot take independently	+	+	
Live-In or Paid Caregiver			
Has no caregiver	-	-	
Has caregiver	+	+	
Expected Home Health Cost			
High	-	-	✓
Low	-	+*	

TABLE V.2 *(continued)*

SOURCE: Appendix Tables B.1 through B.8.

NOTE: ✓✓ means differences in demonstration home health use within subgroups and across subgroups were statistically significant at the .05 level.
✓ means convincing, though weaker, evidence of subgroup differences.

*Subgroup impact significantly different from zero at the .10 level, two-tailed test.
**Subgroup impact significantly different from zero at the .05 level, two-tailed test.
***Subgroup impact significantly different from zero at the .01 level. two-tailed test.

 means the difference between subgroup impacts is significantly different from zero at the .05 level.

pattern of reduced service use for subgroups of patients who were able to respond more effectively to self-care teaching. (The greater difference for patients of agencies with low-use practice patterns, however, seems to have been a chance occurrence, since it was not accompanied by greater differences in demonstration home health visits or emergency room or inpatient service use.)

Overall, during the year following home health admission, patients of treatment group agencies were somewhat more likely than their control counterparts to use nondemonstration home health services. This trend culminated in a 17 percent increase in the proportion of treatment group agency patients admitted to a nondemonstration home health agency during the year following a demonstration admission. We observed no increase in adverse outcomes reflected in more nondemonstration visits or more use of other services; therefore, we concluded that this increase in nondemonstration admissions did not reflect inappropriate discharges from demonstration agencies. We attributed the increased likelihood of having a nondemonstration admission to the fact that treatment group agencies generally discharged patients earlier than control agencies (providing more opportunity for nondemonstration home health use) and to the fact that patients from a few treatment group agencies had unusually high rates of nondemonstration home health use.

For the most part, our subgroup analysis showed that treatment agency patients were more likely than control agency patients to be admitted to a nondemonstration agency, regardless of subgroup affiliation. However, increases in the likelihood of a nondemonstration home health admission were larger for some subgroups: patients of low-use practice pattern agencies, patients of small agencies, and patients with low expected home health costs. Only the increase for small agencies is consistent with our hypothesis: small treatment group home health agencies cut visits more sharply (relative to small control agencies) than did large agencies.

3. Medicare Part A and Part B Reimbursement

Overall, we found that prospective payment had no effect on total Part A or total Part B Medicare spending; this finding was generally evident in subgroup impact estimates. (See Table V.3.) There were three exceptions, however. First, among agencies with costs above the limits, Part A expenditures were 17 percent lower for patients of treatment agencies than for patients of similar control agencies. (This reduction was consistent with small reductions in the use of inpatient and emergency room services for patients of agencies above the cost limits.) Second, among for-profit agencies, patients from treatment agencies had somewhat lower Part B spending, whereas among nonprofit agencies, patients from treatment agencies had somewhat higher Part B spending. Third, among patients who could take medications independently, those from treatment agencies spent 15 percent less on Part B services than similar control agency patients. Again, none of these subgroup differences was consistent with our expectations based on differences in (demonstration) home health visit reductions. However, the treatment-control difference for patients who can manage oral medications was consistent with the pattern of differences in Part A service use.

C. CONCLUSION

We observed no patterns of increased Medicare service use and reimbursement for subgroups of patients who experienced the deepest reductions in home health visits. In fact, the only such increase in service use was an increase in the likelihood of a nondemonstration home health admission for patients of small agencies. Small treatment group agencies reduced home health visits by 30 percent (compared with a 24 percent reduction overall). Among patients of small treatment agencies, 26 percent had a nondemonstration admission during the year following a demonstration admission, compared with only 19 percent of their control counterparts (an increase of 37 percent, compared with 17 percent overall).

TABLE V.3

SUMMARY OF ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN MEDICARE PART A AND B REIMBURSEMENT DURING MONTHS 1 THROUGH 12, BY SUBGROUP

	Medicare Part A Reimbursement (Dollars)	Medicare Part B Reimbursement (Dollars)	Number of Demonstration Home Health Visits
Overall Control Mean/Estimated Difference	$11,292/-168	$4,864/-37	76/-18***
Agency Subgroups			
Practice Pattern			
High-use	+	+	✓✓
Low-use	-	-	
Profit Status			
For-profit	-**	-*	
Nonprofit	+	+*	
Size			
Small	+	+	✓
Large	-	-	
Auspice			
Hospital-based	-	+	
Freestanding	-	-	✓
Cost			
Under limit	+	+	
Over limit	-**	-	✓
Patient Subgroups			
Management of Oral Medications			
Can take independently	-**	-**	
Cannot take independently	-	+	
Live-In or Paid Caregiver			
Has no caregiver	-***	-	
Has caregiver	-	-	
Expected Home Health Cost			
High	-**	-	✓
Low	-	-	

TABLE V.3 *(continued)*

SOURCE: Appendix Tables B.1 through B.8.

NOTE: ✓✓ means differences in demonstration home health use within subgroups and across subgroups were statistically significant at the .05 level.
✓ means convincing, though weaker, evidence of subgroup differences.

*Subgroup impact significantly different from zero at the .10 level, two-tailed test.
**Subgroup impact significantly different from zero at the .05 level, two-tailed test.
***Subgroup impact significantly different from zero at the .01 level, two-tailed test.

▒ means the difference between subgroup impacts is significantly different from zero at the .05 level.

Of the 48 subgroup impact comparisons we conducted (six outcomes times eight subgroup characteristics), 10 (or 21 percent of the comparisons) were statistically significant at the 5 percent level. This is more than we would expect to see by chance. However, in order to have unequivocal evidence that subgroup differences were the result of systematic changes in agency behavior under the demonstration, we would expect to see consistent statistically significant impact differences across outcomes for particular subgroups of agencies or patients (even if the changes did not result in agencies differentially reducing visits). For example, if nonprofit treatment group agencies were more successful than for-profit agencies in improving patient education (even if they did not succeed in reducing home health visits more), patients of nonprofit agencies might still have better health outcomes, as reflected in reductions across the board in the use of other Medicare services. However, no such consistent pattern emerged from our subgroup analysis. In the following chart, shading denotes particular outcome/characteristic comparisons for which we observed a statistically significant subgroup difference:

	Emergency Room Encounters	Inpatient Admissions	Skilled Nursing Facility Admission	Nondemonstration Home Health Use	Part A Reimbursement	Part B Reimbursement
Practice Pattern						
Profit Status						
Size						
Auspice						
Cost						
Medication Management						
Caregiver						
Expected Cost						

At most, for a particular subgroup characteristic, we see statistically significant differences in impact estimates for two outcomes. This occurs for just two characteristics (practice pattern and medication management). For the other six characteristics in our analysis, we see a difference in estimates for only one outcome.

We do, however, find suggestive evidence that treatment agencies may have been successful in improving self-care teaching for patients who were relatively healthier or who had fewer physical or cognitive limitations (namely, those who could manage their oral medications, those who did not have a consistently available caregiver, and those served by hospital-based agencies). For such patients we observe a pattern of larger, negative, treatment-control differences in emergency room, inpatient, and skilled nursing facility service use (though few are statistically significant), in spite of the fact that demonstration home health agencies were not able to reduce the number of visits they provided to them.

Our subgroup impact estimates reinforce our overall conclusion that the reductions in home health visits observed under the demonstration did not adversely affect patients or shift costs in ways reflected in other types of Medicare service use. (The exception was an increase in the use of nondemonstration home health service that seemed to result from earlier, but appropriate, discharge from demonstration agencies.) All the individual subgroup estimates that were significant at the five percent level (other than those for nondemonstration home health use) were negative, repudiating even a hint of an increase in other Medicare service use.

VI. CONCLUSIONS

Per-episode prospective payment for home health services resulted in substantial reductions in the provision of home health visits and the length of home health episodes. However, we found no evidence that these reductions adversely affected patients or shifted care to other settings, even for those patients for whom visits were reduced most.

A. FINDINGS

Treatment group agencies reduced visits by 25 percent during the year following home health admission, primarily through earlier discharge. For patients admitted during the first demonstration year, visits were reduced by 17 percent during the at-risk period but by 35 percent during the latter eight months of the follow-up year. Similar reductions continued for patients admitted during the second demonstration year and were apparent for most subgroups of agencies and patients included in our analyses. Agencies that historically provided more visits than average, however, did make deeper cuts (relative to agencies with histories of fewer visits), mainly by cutting more aide visits (Trenholm 1998).

We found no evidence that these reductions in home health visits adversely affected patient outcomes or shifted care in ways that were reflected in the increased use of other Medicare services in either the first or second demonstration year. In fact, there is some evidence that efforts to facilitate earlier discharge from home health (perhaps by improving the teaching of self-care to patients) may have led to modest reductions during the follow-up year in the use of emergency room services and, to a lesser extent, inpatient service use.

We also found no evidence that reductions in home health visits led patients to shift their care to services provided by skilled nursing facilities, hospices, or nondemonstration home health

agencies. We found no treatment-control differences in the use of or reimbursement for skilled nursing facilities or hospices. In addition, although we observed a higher rate of nondemonstration home health use for treatment agency patients, we do not believe it resulted from inappropriately early discharges. If such discharges had been inappropriate, we would have expected to see increases in emergency room, inpatient, or skilled nursing facility service use. We might also have expected to see nondemonstration agencies providing more visits to treatment agency patients (relative to control agency patients) to compensate for these discharges. We saw no such increases, however.

The higher rate of nondemonstration home health admission among treatment agency patients seems to have been related to two factors. First, because treatment agency patients were discharged earlier than control patients from demonstration agencies, they had a greater window of opportunity to have nondemonstration agency admissions in the follow-up year. That is, control agency patients still receiving demonstration home health services could not have been admitted to other home health agencies (except in rare instances of patient sharing), whereas patients discharged earlier by treatment agencies were at risk of such admissions. Second, a few treatment group agencies whose patients had unusually high nondemonstration admission rates (but *not* unusually low demonstration home health episode length or number of visits) seemed to have driven the treatment-control difference.

Because per-episode prospective payment had modest, but offsetting, effects on the use of Medicare services (small reductions in emergency room and inpatient use but a modest increase in nondemonstration home health use), it had no effect on overall Medicare spending. Medicare expenditures (exclusive of those for demonstration home health services) averaged roughly $16,000 over the year following a demonstration home health admission for patients of both treatment and

control group agencies. Payment for home health services provided by *demonstration agencies* appeared to have been higher for control agency patients over the year: $5,050, compared with $4,439 for treatment agency patients. This was due to the earlier discharge of treatment agency patients: fewer treatment agency patients received demonstration agency services beyond the at-risk period when treatment agencies would have been paid on a per-visit basis. Thus, total Medicare spending for treatment agency patients was somewhat lower than that for their control group counterparts.

B. LIMITATIONS

Our study has some limitations, despite the robustness of its conclusion that the reduction in home health visits observed under per-episode prospective payment did not lead to adverse patient outcomes or cost shifting as reflected in the use of other Medicare services. Perhaps the most important potential limitation is the extent to which our findings may be generalized to home health agencies nationwide. As in any voluntary demonstration, the agencies that chose to participate may reflect a group better able to reduce visits while maintaining care quality. If they did, our demonstration impacts may not accurately reflect changes in Medicare service use that would take place nationally under a similar payment system.

However, we believe that the results of this analysis can be generalized because overall findings and findings for different subgroups of patients were largely consistent. Agencies of differing size, auspice, or practice pattern might be expected to differ in their ability to cut visits while maintaining care quality. However, even patients of agencies that made the deepest cuts to visits did not appear to suffer adverse outcomes, as reflected in their use of other Medicare services. Thus, even if the mix of demonstration agencies differed from agencies nationwide, we might expect a nationwide program to be able to substantially reduce home health visits while not adversely affecting patients

so that they increased their use of other Medicare services. However, if home health agencies nationwide made deeper cuts to visits than we observed under the demonstration, it is unclear at what point such cuts would lead to adverse patient outcomes.

A second limitation, related to the issue of generalizability, is that the national program of prospective payment differs from the one implemented for the demonstration. For example, agencies are not protected from incurring financial losses. This could lead some agencies to respond more aggressively to the program incentive to reduce visits or costs, to be more certain of avoiding losses. If agencies make larger reductions in visits than those observed under this demonstration, at some point these reductions are likely to have an adverse effect on patients. However, no research has been done to determine the critical threshold for such reductions.

A final limitation is that this report does not provide information on all the consequences of reducing services, only on those serious enough to be reflected in the use of other Medicare services. Later reports will examine the effects of the demonstration on other aspects of care quality, patient selection and retention, and the use of informal care and services paid by funders other than Medicare.[1] In addition, we will examine the impacts of the demonstration on agency cost per visit and cost per episode, as well as on profit and loss. A final report will combine the findings across outcome measures.

C. SUMMARY

The evidence this analysis provides clearly shows that the reductions in home health visits observed under the per-episode prospective payment demonstration did not adversely affect patients in ways that were reflected in the increased use of other Medicare services or cause patients to shift

[1]The draft report on the effect of per-episode payment on care quality suggested that the demonstration did not affect quality (Chen 1999).

care to other settings. However, the analysis also underscores the vulnerability of Medicare home health patients. More than half the patients in our analysis were admitted to a hospital during the year following their demonstration home health admission, compared with just under 20 percent of all Medicare enrollees in 1996 (U.S. Social Security Administration 1998). Total Medicare expenditures for patients of demonstration agencies were over $20,000 during the year, more than quadruple the spending for all enrollees during 1996 (Health Care Financing Administration 1998). In addition, a recent AARP study found that 43 percent of Medicare beneficiaries who had used health home services in 1997 had at least one limitation in daily living activities, and 33 percent lived alone, compared with 10 and 25 percent, respectively, of all Medicare enrollees. Their out-of-pocket health care spending was 27 percent of their income, compared with just 18 percent for all Medicare enrollees (Foley et al. 1998).

These statistics paint a picture of the Medicare home health patient population as much more medically fragile, functionally limited, and socially isolated than the typical Medicare enrollee, as well as having higher out-of-pocket health care costs. Therefore, while it seems clear that the reduction in home health visits observed under the demonstration did not increase patients' use of other Medicare services, it is unclear how much larger the reduction could have been without doing harm. Because Medicare home health patients are so vulnerable, caution should accompany the setting of policy parameters that could induce agencies to cut home health visits even more deeply. Moreover, now that prospective payment has become law, as agencies develop more experience with it, HCFA will need to monitor both agencies' responses to the incentive to cut visits and the effect of such cuts on beneficiaries and their use of other Medicare services.

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APPENDIX A

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT DURING DEMONSTRATION YEAR 2

TABLE A.1

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN EMERGENCY ROOM USE DURING DEMONSTRATION YEAR 2

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Whether Any Emergency Room Encounter Resulting in Admission (Percentage)	20.7	−0.8 (0.48)	13.8	−0.8 (0.33)	29.9	−1.1 (0.30)
Number of Emergency Room Encounters Resulting in Admission	0.27	−0.00 (0.99)	0.18	−0.01 (0.51)	0.46	−0.02 (0.39)
Whether Any Emergency Room Encounter Not Resulting in Admission (Percentage)	19.4	−2.4** (0.02)	13.6	−1.3 (0.17)	27.0	−1.8 (0.24)
Number of Emergency Room Encounters Not Resulting in Admission	0.28	−0.03 (0.17)	0.19	−0.01 (0.53)	0.46	−0.03 (0.35)
Whether Any Emergency Room Encounters (Percentage)	33.7	−2.6* (0.09)	23.0	−1.3 (0.25)	45.3	−1.4 (0.47)
Total Number of Emergency Room Encounters	0.55	−0.02 (0.46)	0.38	−0.02 (0.42)	0.92	−0.04 (0.30)
Number of Patients						
Treatment		18,175		11,389		11,389
Control		17,659		12,391		12,391

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" and "any visit" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

TABLE A.1 *(continued)*

[a]The *p*-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.2

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN INPATIENT HOSPITAL USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 2

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Whether Any Admission (Percentage)	31.3	-1.2 (0.21)	20.6	-0.2 (0.88)	43.0	-1.0 (0.39)
Number of Admissions	0.48	-0.0 (0.58)	0.32	-0.0 (0.80)	0.81	-0.0 (0.24)
Number of Days	3.8	-0.3 (0.15)	2.7	-0.2 (0.28)	6.7	-0.8** (0.02)
Reimbursement (Dollars)[b]	3,622	-275 (0.23)	2,521	-214 (0.26)	6,256	-614* (0.07)
Number of Patients						
Treatment		18,175		11,389		11,389
Control		17,659		12,391		12,391

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly difference from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for inpatient services under Medicare Part B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.3

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN SKILLED NURSING FACILITY AND HOSPICE USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 2

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Skilled Nursing Facility						
Whether Any Admission (Percentage)	9.8	0.3 (0.70)	6.8	0.4 (0.51)	14.9	0.7 (0.47)
Number of Admissions	0.12	0.01 (0.61)	0.08	0.01 (0.35)	0.21	0.01 (0.40)
Number of Days	2.2	0.2 (-0.48)	2.1	-0.0 (1.00)	4.4	0.2 (0.69)
Reimbursement (Dollars)[b]	714	-13 (0.86)	654	-1 (0.99)	1387	-31 (0.81)
Hospice						
Whether Any Admission (Percentage)	3.0	-0.5 (0.31)	1.7	0.0 (0.90)	4.6	-0.6 (0.33)
Number of Days	0.66	-0.04 (0.77)	0.50	0.21* (0.06)	1.14	0.26 (0.23)
Reimbursement (Dollars)[b]	81	-9 (0.54)	64	19* (0.10)	141	21 (0.37)

TABLE A.3 *(continued)*

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Number of Patients						
Treatment		18,175		11,389		11,389
Control		17,659		12,391		12,391

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for inpatient services under Medicare Part B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.4

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN NONDEMONSTRATION HOME HEALTH USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 2

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Whether Any Admission (Percentage)[b]	8.1	1.0 (0.26)	10.2	1.2 (0.29)	13.3	1.2 (0.35)
Number of Visits[b]	3.67	0.10 (0.89)	5.50	0.69 (0.54)	9.26	0.97 (0.59)
Reimbursement (Dollars)[b]	263	27 (0.55)	381	69 (0.37)	642	124 (0.30)
Number of Patients						
Treatment		18,175		11,389		11,389
Control		17,659		12,391		12,391

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Reimbursement is composed primarily of payments under Medicare Part A but also includes a small number of payments for inpatient services under Medicare Part B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.5

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN PART B USE AND REIMBURSEMENT DURING DEMONSTRATION YEAR 2

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Outpatient Hospital[b]						
Whether Any Services (Percentage)	66.6	−1.5 0.41	51.2	−0.1 (0.96)	76.0	−0.3 (0.87)
Reimbursement (Dollars)	523	9 (0.72)	399	7 (0.80)	889	23 (0.60)
Physician and Other Practitioner						
Whether Any Visit (Percentage)	90.9	−0.4 (0.54)	79.4	0.8 (0.37)	94.0	0.6 (0.40)
Reimbursement (Dollars)	905	−17 (0.67)	642	−6 (0.86)	1,567	−28 (0.70)
Durable Medical Equipment						
Whether Any Purchase (Percentage)	46.2	−1.6 0.19	32.2	−1.4 (0.34)	52.5	−2.3 (0.17)
Reimbursement (Dollars)	294	4 (0.78)	221	6 (0.76)	531	−6 (0.85)

TABLE A.5 *(continued)*

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Other Part B Services[c]						
Whether Any Use (Percentage)	80.8	−0.7	65.9	−0.6	89.2	0.6
		(0.62)		(0.63)		(0.53)
Reimbursement (Dollars)	301	1	218	−7	516	−2
		(0.95)		(0.61)		(0.93)
Number of Patients						
Treatment		18,175		11,389		11,389
Control		17,659		12,391		12,391

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission," "any visit," "any purchase," and "any use" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Includes both emergency and nonemergency visits to outpatient hospitals.

[c]Includes diagnostic laboratory and radiology services (including pathologist and radiologist services) from nonhospital providers, supplies and devices, mammography, ambulance, covered medications, blood, and vaccines.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.6

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN TOTAL MEDICARE REIMBURSEMENT DURING DEMONSTRATION YEAR 2

	Months 1 Through 4		Months 5 Through 8		Months 1 Through 8	
	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]	Control Group Mean	Difference (p-Value)[a]
Total Medicare Part A Reimbursement[b]	4,730	−255 (0.33)	3,633	−95 (0.67)	8,490	−455 (0.25)
Total Medicare Part B Reimbursement	2,023	−6 (0.93)	1,480	−3 (0.97)	3,502	−14 (0.91)
Total Medicare Reimbursement[c]	6,752	−246 (0.41)	5,113	−83 (0.76)	11,992	−435 (0.35)
Number of Patients						
Treatment	18,175		11,389		11,389	
Control	17,659		12,391		12,391	

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission," "any visit," "any purchase," and "any use" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

[a]The p-values (tests that the coefficients on treatment status in the models were significantly different from zero) were based on estimated standard errors that account for the effects of clustering and weighting.

[b]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

[c]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

APPENDIX B

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DURING DEMONSTRATION YEAR 1, BY SUBGROUP

TABLE B.1

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY AGENCY PRACTICE PATTERN[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
High-use practice pattern	1.09	−0.03	(0.60)
Low-use practice pattern	1.09	−0.06	(0.16)
Difference between subgroups		0.03	(0.62)
Total Number of Emergency Room Encounters			
High-use practice pattern	1.31	−0.12**	(0.02)
Low-use practice pattern	1.22	−0.07	(0.16)
Difference between subgroups		−0.05	(0.55)
Whether Any Skilled Nursing Facility Admission			
High-use practice pattern	19.6	1.6	(0.17)
Low-use practice pattern	20.5	−2.6**	(0.02)
Difference between subgroups		4.2**	(0.02)
Whether Any Nondemonstration Home Health Admissions			
High-use practice pattern	17.4	0.3	(0.93)
Low-use practice pattern	16.1	6.0***	(0.00)
Difference between subgroups		−5.7***	(0.01)
Total Medicare Part A Reimbursement (Dollars)[e]			
High-use practice pattern	10,846	224	(0.62)
Low-use practice pattern	11,988	−687	(0.18)
Difference between subgroups		911	(0.23)
Total Medicare Part B Reimbursement (Dollars)			
High-use practice pattern	4,689	14	(0.71)
Low-use practice pattern	5,137	−35	(0.55)
Difference between subgroups		49	(0.52)

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE B.1 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

	Sample Size			
	High-Use Practice Pattern		Low-Use Practice Pattern	
	Patients	Agencies	Patients	Agencies
Treatment Group	14,606	18	16,593	28
Control Group	9,457	25	16,352	16

[a]An agency is defined as having a high-use practice pattern if its (case-mix adjusted) number of visits per episode in the base (predemonstration) period was above the median for all agencies in the sample. Otherwise, it is defined as having a low-use practice pattern.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.2

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY AGENCY PROFIT STATUS[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
For-profit	1.11	−0.10**	(0.02)
Nonprofit	1.07	0.01	(0.78)
Difference between subgroups		−0.11*	(0.08)
Total Number of Emergency Room Encounters			
For-profit	1.31	−0.09*	(0.09)
Nonprofit	1.23	−0.10**	(0.05)
Difference between subgroups		0.01	(0.92)
Whether Any Skilled Nursing Facility Admission			
For-profit	19.8	−1.3	(0.33)
Nonprofit	20.1	0.4	(0.70)
Difference between subgroups		−1.7	(0.35)
Whether Any Nondemonstration Home Health Admissions			
For-profit	19.2	2.5	(0.21)
Nonprofit	14.3	3.6**	(0.02)
Difference between subgroups		−1.1	(0.55)
Total Medicare Part A Reimbursement (Dollars)[e]			
For-profit	12,005	−1,036**	(0.04)
Nonprofit	10,543	544	(0.30)
Difference between subgroups		−1,580*	(0.06)
Total Medicare Part B Reimbursement (Dollars)			
For-profit	5,134	−355*	(0.10)
Nonprofit	4,581	326*	(0.10)
Difference between subgroups		−681**	(0.04)

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE B.2 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

	Sample Size			
	For-Profit		Non-Profit	
	Patients	Agencies	Patients	Agencies
Treatment Group	6,439	22	24,760	24
Control Group	6,720	21	19,089	20

[a]An agency is defined as for-profit or nonprofit according to its characterization on its base-year cost report.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.3

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY AGENCY SIZE[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
Small agencies	1.01	-0.03	(0.59)
Large agencies	1.11	-0.05	(0.12)
Difference between subgroups		0.02	(0.79)
Total Number of Emergency Room Encounters			
Small agencies	1.28	-0.12	(0.20)
Large agencies	1.27	-0.08***	(0.01)
Difference between subgroups		-0.04	(0.73)
Whether Any Skilled Nursing Facility Admission			
Small agencies	18.1	-2.4	(0.17)
Large agencies	20.4	0.2	(0.78)
Difference between subgroups		-2.6	(0.16)
Whether Any Nondemonstration Home Health Admissions			
Small agencies	19.0	7.0***	(0.00)
Large agencies	16.3	1.3	(0.25)
Difference between subgroups		5.7**	(0.02)
Total Medicare Part A Reimbursement (Dollars)[e]			
Small agencies	10,346	306	(0.62)
Large agencies	11,521	-444	(0.24)
Difference between subgroups		750	(0.34)
Total Medicare Part B Reimbursement (Dollars)			
Small agencies	4,352	425	(0.25)
Large agencies	4,988	-177	(0.20)
Difference between subgroups		602*	(0.10)

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE B.3 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

	Sample Size			
	Small		Large	
	Patients	Agencies	Patients	Agencies
Treatment Group	2,404	16	28,795	30
Control Group	754	8	25,055	33

[a]Agency size is defined by the number of visits provided in the agency's base year, according to its base-year cost report. An agency is categorized as small if it provided few than 30,000 in that year; otherwise, it is categorized as large.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.4

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY AGENCY AUSPICE[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
Hospital-based	1.08	−0.11	(0.17)
Freestanding	1.09	−0.03	(0.27)
Difference between subgroups		−0.08	(0.39)
Total Number of Emergency Room Encounters			
Hospital-based	1.24	−0.19	(0.11)
Freestanding	1.28	−0.08**	(0.02)
Difference between subgroups		−0.11	(0.37)
Whether Any Skilled Nursing Facility Admission			
Hospital-based	20.3	−6.5**	(0.02)
Freestanding	19.9	0.3	(0.64)
Difference between subgroups		−6.8***	(0.01)
Whether Any Nondemonstration Home Health Admissions			
Hospital-based	13.5	1.0	(0.79)
Freestanding	17.4	3.2***	(0.00)
Difference between subgroups		−2.2	(0.52)
Total Medicare Part A Reimbursement (Dollars)[e]			
Hospital-based	11,274	−1,489	(0.19)
Freestanding	11,295	−74	(0.81)
Difference between subgroups		−1,415	(0.24)
Total Medicare Part B Reimbursement (Dollars)			
Hospital-based	4,974	267	(0.99)
Freestanding	4,845	−47	(0.79)
Difference between subgroups		314	(0.93)

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE B.4 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

Sample Size

	Hospital-Based		Freestanding	
	Patients	Agencies	Patients	Agencies
Treatment Group	1,728	4	29,471	42
Control Group	4,271	6	21,538	35

[a]An agency is defined as hospital-based or freestanding according to its characterization on its base-year cost report.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.5

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY AGENCY COST LEVEL[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
Under cost limit	1.08	−0.04	(0.23)
Over cost limit	1.15	−0.09	(0.28)
Difference between subgroups		0.05	(0.56)
Total Number of Emergency Room Encounters			
Under cost limit	1.28	−0.07**	(0.05)
Over cost limit	1.21	−0.20**	(0.02)
Difference between subgroups		0.13	(0.20)
Whether Any Skilled Nursing Facility Admission			
Under cost limit	19.6	−0.9	(0.32)
Over cost limit	21.9	2.1	(0.28)
Difference between subgroups		−3.0	(0.19)
Whether Any Nondemonstration Home Health Admissions			
Under cost limit	16.4	3.6***	(0.01)
Over cost limit	19.4	−0.9	(0.69)
Difference between subgroups		4.5	(0.14)
Total Medicare Part A Reimbursement (Dollars)[e]			
Under cost limit	10,876	129	(0.69)
Over cost limit	13,716	−2,322**	(0.04)
Difference between subgroups		2,451**	(0.04)
Total Medicare Part B Reimbursement (Dollars)			
Under cost limit	4,687	50	(0.94)
Over cost limit	5,899	−354	(0.38)
Difference between subgroups		404	(0.43)

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE B.5 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

	Sample Size			
	Under Cost Limit		Over Cost Limit	
	Patients	Agencies	Patients	Agencies
Treatment Group	28,630	39	2,569	7
Control Group	21,530	35	4,279	6

[a]An agency is defined as under or over the Medicare cost limits according to its characterization on its base-year cost report.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.6

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY WHETHER THE PATIENT CAN TAKE ORAL MEDICATIONS INDEPENDENTLY[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
Can take medications independently	0.98	−0.08	(0.16)
Cannot take medications independently	1.15	−0.02	(0.67)
Difference between subgroups		−0.06	(0.48)
Total Number of Emergency Room Encounters			
Can take medications independently	1.13	−0.15***	(0.01)
Cannot take medications independently	1.35	−0.06	(0.31)
Difference between subgroups		−0.09	(0.23)
Whether Any Skilled Nursing Facility Admission			
Can take medications independently	15.9	−2.5*	(0.06)
Cannot take medications independently	21.4	1.2	(0.39)
Difference between subgroups		−3.7**	(0.02)
Whether Any Nondemonstration Home Health Admissions			
Can take medications independently	14.6	1.5	(0.45)
Cannot take medications independently	20.2	0.4	(0.81)
Difference between subgroups		1.1	(0.61)
Total Medicare Part A Reimbursement (Dollars)[e]			
Can take medications independently	10,080	−1,554**	(0.02)
Cannot take medications independently	12,756	−567	(0.40)
Difference between subgroups		−987	(0.32)
Total Medicare Part B Reimbursement (Dollars)			
Can take medications independently	5,144	−523**	(0.05)
Cannot take medications independently	4,766	145	(0.39)
Difference between subgroups		−668**	(0.05)

TABLE B.6 *(continued)*

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

Sample Size

	Can Take Oral Medications Independently		Cannot Take Oral Medications Independently	
	Patients	Agencies	Patients	Agencies
Treatment	6,328	41	6,712	41
Control	2,987	36	3,514	36

[a]Ability to take oral medications independently is based on agency nurse assessment at admission to home health, as recorded as part of demonstration quality assurance procedures.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.7

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY WHETHER THE PATIENT HAS CAREGIVER[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
Has no caregiver	1.09	−0.15***	(0.00)
Has caregiver	1.06	0.01	(0.90)
Difference between subgroups		−0.16**	(0.03)
Total Number of Emergency Room Encounters			
Has no caregiver	1.24	−0.14**	(0.04)
Has caregiver	1.27	−0.09*	(0.07)
Difference between subgroups		−0.05	(0.50)
Whether Any Skilled Nursing Facility Admission			
Has no caregiver	19.9	−1.5	(0.34)
Has caregiver	18.5	0.1	(0.93)
Difference between subgroups		−1.6	(0.40)
Whether Any Nondemonstration Home Health Admissions			
Has no caregiver	17.9	−0.0	(0.98)
Has caregiver	17.8	1.3	(0.36)
Difference between subgroups		−1.3	(0.45)
Total Medicare Part A Reimbursement (Dollars)[e]			
Has no caregiver	11,189	−1,548***	(0.01)
Has caregiver	11,904	−773	(0.22)
Difference between subgroups		−775	(0.34)
Total Medicare Part B Reimbursement (Dollars)			
Has no caregiver	4,674	−325	(0.14)
Has caregiver	5,093	−54	(0.76)
Difference between subgroups		−271	(0.35)

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

TABLE B.7 *(continued)*

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

Sample Size

	Has No Caregiver		Has Caregiver	
	Patients	Agencies	Patients	Agencies
Treatment Group	5,656	41	7,384	41
Control Group	2,541	36	3,960	36

[a]Availability of caregiver is based on agency nurse assessment at admission to home health about whether the patient has a paid caregiver or a live-in informal caregiver, or lives in an assisted setting, as recorded as part of demonstration quality assurance procedures.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE B.8

ESTIMATED DIFFERENCES BETWEEN PER-EPISODE PAYMENT AND COST REIMBURSEMENT IN KEY OUTCOMES, DEMONSTRATION YEAR 1, BY EXPECTED PATIENT HOME HEALTH COST[a]

In the Year After Admission[b]	Control Group Mean	Difference[c]	(p-Value)[d]
Number of Inpatient Admissions			
High Expected Costs	1.15	−0.02	(0.71)
Low Expected Costs	1.04	−0.07*	(0.10)
Difference between subgroups		0.05	(0.44)
Total Number of Emergency Room Encounters			
High Expected Costs	1.29	−0.04	(0.61)
Low Expected Costs	1.23	−0.14***	(0.01)
Difference between subgroups:		0.10	(0.19)
Whether Any Skilled Nursing Facility Admission			
High Expected Costs	22.5	−1.0	(0.53)
Low Expected Costs	17.2	−0.2	(0.88)
Difference between subgroups		−0.8	(0.65)
Whether Any Nondemonstration Home Health Admissions			
High Expected Costs	22.9	−1.9	(0.34)
Low Expected Costs	15.1	2.3*	(0.06)
Difference between subgroups		−4.2**	(0.02)
Total Medicare Part A Reimbursement (Dollars)[e]			
High Expected Costs	13,819	−1,765**	(0.04)
Low Expected Costs	10,431	−743	(0.16)
Difference between subgroups		−1,022	(0.32)
Total Medicare Part B Reimbursement (Dollars)			
High Expected Costs	5,266	−153	(0.46)
Low Expected Costs	4,743	−168	(0.32)
Difference between subgroups		15	(0.95)

TABLE B.8 *(continued)*

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

NOTE: The differences presented for "any admission" were estimated using a logit model; other differences were estimated using ordinary least squares (OLS). "Differences" are the average difference between the expected value for all observations if they were patients of treatment group agencies and the expected value for all observations if they were patients of control group agencies. (For OLS results, this difference is the coefficient on the treatment status indicator.) Observations were weighted so that each agency is represented equally.

Sample Size

	High Expected Costs		Low Expected Costs	
	Patients	Agencies	Patients	Agencies
Treatment Group	3,835	41	9,205	41
Control Group	1,941	36	4,560	36

[a]A patient is defined as having high expected costs if his or her predicted reimbursement for home health services in the year after admission is in the top quartile for all patients in the sample. Otherwise, the patient is defined as having low expected costs.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through OLS and logit models) to control for preexisting differences between treatment and control agencies.

[d]The *p*-value for each subgroup corresponds to a test of whether the treatment-control difference within the subgroup is different from zero. The *p*-value for "difference between subgroups" corresponds to a test of whether the difference between subgroup differences is different from zero. All *p*-values are based on standard errors that account for the effects of clustering and weighting.

[e]Excludes Medicare Part A reimbursement for home health services provided by demonstration agencies. Includes reimbursement for Part B demonstration home health, as well as inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

APPENDIX C

ESTIMATED COEFFICIENTS FOR KEY OUTCOMES DURING DEMONSTRATION YEAR 1

TABLE C.1

ESTIMATED COEFFICIENTS, NUMBER OF INPATIENT ADMISSIONS, AND NUMBER OF EMERGENCY ROOM CONTACTS DURING THE YEAR AFTER ADMISSION, DEMONSTRATION YEAR 1

	Number of Inpatient Admissions (*p*-Value)	Number of Emergency Room Encounters (*p*-Value)
Intercept	0.69*** (0.00)	0.85*** (0.00)
Agency Received Prospective Payment	−0.04 (0.21)	−0.09*** (0.01)
Age		
Younger than 65	0.27*** (0.00)	0.40*** (0.00)
75 to 84	0.07** (0.03)	0.08*** (0.01)
85 or older	0.04 (0.17)	0.11*** (0.01)
White	0.06 (0.20)	0.03 (0.57)
Female	−0.04* (0.10)	−0.05 (0.24)
Original Reason for Medicare: Old Age	−0.14*** (0.00)	−0.14*** (0.01)
Had Medicare Buy-In	0.14*** (0.00)	0.29*** (0.00)
Had Medicare for Less than Six Months	0.15 (0.19)	0.12 (0.29)
Enrolled in an HMO at Some Time in the Six Months Before Admission	0.08 (0.43)	0.08 (0.55)
Had Medicare as a Second Payer at Some Time in the Six Months Before Admission	0.10 (0.48)	−0.15 (0.43)
Medical Conditions		
Cancer	0.06 (0.11)	−0.04 (0.32)
Diabetes	0.29*** (0.00)	0.20*** (0.00)
Cerebrovascular accident (stroke)	−0.02 (0.48)	0.03 (0.51)
Decubiti stage 3 or 4	0.15** (0.03)	0.00 (0.97)

TABLE C.1 *(continued)*

	Number of Inpatient Admissions (*p*-Value)	Number of Emergency Room Encounters (*p*-Value)
Need for Complicated Wound Care [a]	−0.03 (0.40)	−0.10 (0.14)
Functional Limitations[b]		
Bathing	0.05 (0.19)	0.09* (0.06)
Eating	0.04 (0.23)	0.08* (0.06)
Dressing	−0.03 (0.33)	−0.02 (0.62)
Toileting	0.02 (0.41)	−0.04 (0.36)
Transferring	0.03 (0.35)	−0.05* (0.08)
Preadmission Location: Hospital	−0.10*** (0.00)	−0.10*** (0.01)
Length of Hospital Stay During Two Weeks Before Home Health (Days)[c]	−0.00 (0.68)	0.00 (1.00)
Any Skilled Nursing Facility Stay During Two Weeks Before Home Health	−0.13*** (0.00)	−0.14*** (0.01)
Total Medicare Part A Reimbursement During Six Months Before Home Health (In Thousands of Dollars) [d]	−0.00 (0.52)	−0.00 (0.96)
Admission Date Between 5/30/95 and 9/30/95	0.00 (0.96)	0.05 (0.31)
Admission Date Between 10/1/95 and 12/31/95	0.02 (0.63)	0.06 (0.34)
Admission Date Between 4/1/96 and 6/30/96	−0.00 (0.94)	0.01 (0.76)
Admission Date Between 7/1/96 and 9/30/96	0.01 (0.64)	0.02 (0.50)
Admission Date Between 10/1/96 and 12/31/96	−0.01 (0.76)	−0.01 (0.77)
For-Profit Agency	0.01 (0.73)	0.09* (0.08)
Hospital-Based Agency	−0.02 (0.63)	−0.06 (0.22)
Chain Member	0.02 (0.59)	0.02 (0.63)

TABLE C.1 *(continued)*

	Number of Inpatient Admissions (*p*-Value)	Number of Emergency Room Encounters (*p*-Value)
Agency Provided Fewer than 30,000 Visits in Base Year	-0.04 (0.20)	-0.08 (0.12)
Ratio of Mean Agency Visits to Mean for All Demonstration Agencies	-0.03 (0.75)	-0.11 (0.16)
Agency State		
Florida	0.05 (0.23)	0.16*** (0.00)
Illinois	0.19*** (0.00)	-0.13** (0.04)
Massachusetts	0.13** (0.03)	0.31*** (0.00)
Texas	0.04 (0.99)	0.12*** (0.01)
Agency Located in Urban Area	0.00 (0.99)	-0.03 (0.70)
County-Level Means		
Number of physicians per 10,000 persons	0.00 (0.38)	0.00 (0.71)
Number of nursing home beds per 100 persons over age 65	-0.01 (0.37)	0.00 (0.79)
Hospital occupancy rate	0.05 (0.73)	0.09 (0.66)
Medicare reimbursement per beneficiary (in thousands of dollars)	-0.03 (0.49)	-0.07 (0.18)
Lagged Value Dependent Variable (During Six Months Before Home Health Admission)	0.37*** (0.00)	0.58*** (0.00)
Number of Episodes	**57,008**	**57,008**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

[a]Patient has wound that requires soaking, irrigation, or debridement.

[b]Patient requires some human assistance with or does not participate in activity.

[c]If patient was not hospitalized within the two weeks before home health, days are set to zero.

[d]Includes reimbursement for inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Medicare Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE C.2

ESTIMATED COEFFICIENTS, ANY SKILLED NURSING FACILITY ADMISSION, AND ANY OTHER HOME HEALTH ADMISSION DURING THE YEAR AFTER ADMISSION, DEMONSTRATION YEAR 1

	Any Skilled Nursing Facility Admission (*p*-Value)	Any Other Home Health Admission (*p*-Value)
Intercept	-2.16*** (0.00)	-2.13*** (0.00)
Agency Received Prospective Payment	-0.01 (0.88)	0.20** (0.02)
Age		
Younger than 65	-0.07 (0.53)	0.04 (0.73)
75 to 84	0.38*** (0.00)	0.12** (0.05)
85 or older	0.63*** (0.00)	0.07 (0.40)
White	0.35*** (0.00)	-0.13** (0.05)
Female	0.03 (0.44)	0.12** (0.04)
Original Reason for Medicare: Old Age	-0.07 (0.39)	-0.15* (0.06)
Had Medicare Buy-In	0.08 (0.25)	0.01 (0.85)
Had Medicare for Less than Six Months	-0.21 (0.23)	-0.20 (0.33)
Enrolled in an HMO at Some Time in the Six Months Before Admission	-0.17 (0.28)	0.23 (0.23)
Had Medicare as a Second Payer at Some Time in the Six Months Before Admission	-0.24 (0.38)	-0.37 (0.30)
Medical Conditions		
Cancer	-0.00 (0.97)	-0.09 (0.18)
Diabetes	0.26*** (0.00)	0.14*** (0.01)
Cerebrovascular accident (stroke)	0.20*** (0.00)	0.24*** (0.00)
Decubiti stage 3 or 4	0.20 (0.12)	-0.05 (0.66)

TABLE C.2 *(continued)*

	Any Skilled Nursing Facility Admission (*p*-Value)	Any Other Home Health Admission (*p*-Value)
Need for Complicated Wound Care[a]	-0.12 (0.19)	0.11 (0.17)
Functional Limitations[b]		
Bathing	0.34*** (0.00)	0.18** (0.04)
Eating	0.08 (0.14)	-0.10 (0.16)
Dressing	-0.03 (0.66)	0.01 (0.85)
Toileting	0.06 (0.27)	0.08 (0.28)
Transferring	0.07 (0.19)	-0.04 (0.55)
Preadmission Location: Hospital	-0.15*** (0.00)	-0.19*** (0.00)
Length of Hospital Stay During Two Weeks Before Home Health (Days)[c]	-0.01* (0.09)	0.01 (0.26)
Any Skilled Nursing Facility Stay During Two Weeks Before Home Health	-0.21*** (0.01)	-0.06 (0.30)
Total Medicare Part A Reimbursement During Six Months Before Home Health (In Thousands of Dollars)[d]	0.00** (0.04)	0.00** (0.02)
Admission Date Between 5/30/95 and 9/30/95	0.08 (0.44)	-0.12 (0.15)
Admission Date Between 10/1/95 and 12/31/95	-0.05 (0.37)	-0.12 (0.11)
Admission Date Between 4/1/96 and 6/30/96	0.00 (0.95)	-0.01 (0.82)
Admission Date Between 7/1/96 and 9/30/96	0.05 (0.37)	0.06 (0.29)
Admission Date Between 10/1/96 and 12/31/96	-0.05 (0.39)	0.12 (0.24)
For-Profit Agency	-0.02 (0.75)	0.17** (0.02)
Hospital-Based Agency	0.00 (0.96)	-0.20* (0.09)
Chain Member	0.06 (0.23)	0.02 (0.85)

TABLE C.2 *(continued)*

	Any Skilled Nursing Facility Admission (*p*-Value)	Any Other Home Health Admission (*p*-Value)
Agency Provided Fewer than 30,000 Visits in Base Year	0.00 (1.00)	0.14 (0.12)
Ratio of Mean Agency Visits to Mean for All Demonstration Agencies	0.04 (0.67)	0.07 (0.62)
Agency State		
Florida	-0.20*** (0.01)	0.31** (0.03)
Illinois	-0.17 (0.11)	-0.23* (0.09)
Massachusetts	0.08 (0.43)	-0.37*** (0.01)
Texas	-0.27*** (0.00)	0.11 (0.37)
Agency Located in Urban Area	0.02 (0.86)	0.14 (0.35)
County-Level Means		
Number of physicians per 10,000 persons	0.00 (0.65)	0.01 (0.23)
Number of nursing home beds per 100 persons over age 65	-0.00 (0.89)	-0.06* (0.06)
Hospital occupancy rate	0.38 (0.29)	-0.84* (0.08)
Medicare reimbursement per beneficiary (in thousands of dollars)	-0.14 (0.14)	0.16 (0.29)
Lagged Value Dependent Variable (During Six Months Before Home Health Admission)	0.61*** (0.00)	0.90*** (0.00)
Number of Episodes	**57,008**	**57,008**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

[a]Patient has wound that requires soaking, irrigation, or debridement.

[b]Patient requires some human assistance with or does not participate in activity.

[c]If patient was not hospitalized within the two weeks before home health, days are set to zero.

[d]Includes reimbursement for inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Medicare Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE C.3

ESTIMATED COEFFICIENTS, MEDICARE PART A REIMBURSEMENT, AND MEDICARE PART B REIMBURSEMENT DURING THE YEAR AFTER ADMISSION, DEMONSTRATION YEAR 1

	Medicare Part A Reimbursement (p-Value)	Medicare Part B Reimbursement (p-Value)
Intercept	3,944 (0.15)	2,869*** (0.00)
Agency Received Prospective Payment	−168 (0.62)	−37 (0.79)
Age		
Younger than 65	2,401*** (0.00)	1,634*** (0.00)
75 to 84	408 (0.29)	−42 (0.70)
85 or older	−3 (1.00)	−189 (0.12)
White	−419 (0.45)	−443** (0.02)
Female	−269 (0.38)	34 (0.73)
Original Reason for Medicare: Old Age	−854 (0.15)	−445*** (0.01)
Had Medicare Buy-In	1,412*** (0.00)	485*** (0.01)
Had Medicare for Less than Six Months	1,998* (0.07)	1,366*** (0.01)
Enrolled in an HMO at Some Time in the Six Months Before Admission	1,535 (0.32)	1,801*** (0.00)
Had Medicare as a Second Payer at Some Time in the Six Months Before Admission	2,449 (0.20)	2,525** (0.02)
Medical Conditions		
Cancer	963** (0.05)	−349 (0.18)
Diabetes	3,490*** (0.00)	986*** (0.00)
Cerebrovascular accident (stroke)	976 (0.12)	316*** (0.01)
Decubiti stage 3 or 4	2,577*** (0.01)	785*** (0.00)

TABLE C.3 *(continued)*

	Medicare Part A Reimbursement (*p*-Value)	Medicare Part B Reimbursement (*p*-Value)
Need for Complicated Wound Care [a]	583 (0.35)	−132 (0.48)
Functional Limitations[b]		
Bathing	919** (0.05)	−56 (0.76)
Eating	1,191*** (0.00)	274** (0.04)
Dressing	5 (0.99)	−245** (0.05)
Toileting	214 (0.56)	−40 (0.72)
Transferring	421 (0.20)	−23 (0.83)
Preadmission Location: Hospital	−545* (0.10)	−336*** (0.00)
Length of Hospital Stay During Two Weeks Before Home Health (Days)[c]	3 (0.92)	5 (0.56)
Any Skilled Nursing Facility Stay During Two Weeks Before Home Health	−1,147*** (0.00)	−379*** (0.01)
Total Medicare Part A Reimbursement During Six Months Before Home Health (In Thousands of Dollars) [d]	185*** (0.00)	−67*** (0.00)
Admission Date Between 5/30/95 and 9/30/95	−652 (0.28)	−247 (0.25)
Admission Date Between 10/1/95 and 12/31/95	56 (0.87)	−99 (0.55)
Admission Date Between 4/1/96 and 6/30/96	488 (0.17)	−80 (0.46)
Admission Date Between 7/1/96 and 9/30/96	6 (0.99)	−147 (0.24)
Admission Date Between 10/1/96 and 12/31/96	556 (0.26)	−81 (0.62)
For-Profit Agency	593 (0.22)	195 (0.22)
Hospital-Based Agency	196 (0.69)	105 (0.53)
Chain Member	−106 (0.80)	−51 (0.68)

TABLE C.3 *(continued)*

	Medicare Part A Reimbursement (*p*-Value)	Medicare Part B Reimbursement (*p*-Value)
Agency Provided Fewer than 30,000 Visits in Base Year	-268 (0.56)	-97 (0.57)
Ratio of Mean Agency Visits to Mean for All Demonstration Agencies	-35 (0.97)	121 (0.66)
Agency State		
Florida	-1,737*** (0.00)	257 (0.32)
Illinois	-2,604*** (0.00)	-306 (0.19)
Massachusetts	-1,474** (0.04)	-149 (0.47)
Texas	-1,782*** (0.00)	52 (0.79)
Agency Located in Urban Area	757 (0.25)	34 (0.82)
County-Level Means		
Number of physicians per 10,000 persons	51* (0.08)	15* (0.09)
Number of nursing home beds per 100 persons over age 65	-144 (0.22)	-101*** (0.00)
Hospital occupancy rate	-732 (0.75)	-846 (0.12)
Medicare reimbursement per beneficiary (in thousands of dollars)	1,271*** (0.01)	150 (0.54)
Lagged Value Dependent Variable (During Six Months Before Home Health Admission)	-	1*** (0.00)
Number of Episodes	**57,008**	**57,008**

SOURCE: Medicare Standard Analytic Files 1995 to 1997.

[a]Patient has wound that requires soaking, irrigation, or debridement.

[b]Patient requires some human assistance with or does not participate in activity.

[c]If patient was not hospitalized within the two weeks before home health, days are set to zero.

[d]Includes reimbursement for inpatient, skilled nursing facility, hospice, and nondemonstration home health paid under Medicare Parts A and B.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.